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CLINICAL LECTURE

BY

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DISEASE OF THE EXTERNAL SURFACE OF THE LIVER, STOMACH, INTESTINES, &c.

Absence of all Symptoms of the Disease, up nearly to the period of Death.

THE first case of which I will speak, Gentlemen, is that of John Milliman, aged 36, admitted February the 19th. He was a short, slim, pale, delicate man, and said, that about three years before, he had fallen down the steps of St. Bride's church, and been taken up senseless. Since then he had complained of occasional pain in the cervical vertebræ,—sometimes down the whole spine, but especially in the cervical vertebræ, and he said that those pains were increased by a change of weather. He represented himself as having been under medical treatment for three months after his fall, and he had also been an out-patient of this hospital. He said that his general health was good, and that he had no other complaint.

I could discover nothing the matter with him, except that the spine about the cervical vertebræ was rather tender. There was no protuberance nor heat, but on rubbing the fingers up and down the cervical vertebræ, there was some little degree of uneasiness experienced by him. He directed my attention to no other part of the body. The only complaint he made was of pain in the neck. With this exception he said he was perfectly well. I did not satisfy myself that there was anything the matter with the cervical vertebræ. It is easy for a person to say that he has pain in any particular situation, and on placing your hand upon any part, it is likewise very easy for him to say it is tender. There was no swelling nor redness; no heat, and he could move himself backwards and forwards, and turn his head round very well. If there was any pain at all, it appeared to me to be *merely* a pain,—that is to say, a sort of neuralgic affection.

Treatment.—A blister was placed over the cervical vertebræ, and he took two drachms of the carbonate of iron three times a day. This was on the 10th of February, and the doses were increased up to the 28th. The medicine was given more frequently, but he was no better. Then on the 29th he said he was much better, and that he had but little pain. At that time, however, his symptoms all at once changed, or, rather, very evident symptoms presented themselves, although he said he was so much better, and had lost all his pain in the back of the neck. He was observed to speak incoherently. He was observed likewise to have become exceedingly thin, and his pulse was very weak. He had been allowed full diet the whole of the time, and now a pint of porter was added to it. In two or three days, that is to say on the 5th of March, he wandered still more in his head. He was observed to keep his bed from weakness, and had become very much emaciated. He then for the first time complained of something somewhere else, but had made no complaint till I was very much struck with his altered appearance, and determined on making an examination of every part of the body. Nothing was found the matter with the chest, but on examining the abdomen, I found that most likely the cause of his sudden emaciation was situated there. I found the abdomen full, very full, and rather firm, and on pressing him in various parts of it, I found he complained of tenderness at the lower part. In this I am sure there was no deception, because he was incoherent, and if he had been disposed to deceive me originally with respect to his neck, being now incoherent, there was no chance of his attempting deception. With respect to the abdomen, he complained of pain regularly when pressed on the lower

part. The circumstance of the abdomen being large, rather confirmed my conviction that there was real pain situated there. He then had an attack of diarrhœa, and passed his fæces in bed, and his pulse became very small and rapid. I ordered a blister to be placed on the abdomen, for he was altogether too weak to allow of any loss of blood. Two grains of opium were given him immediately, together with an infusion of catechu, and the diarrhœa was stopped, but the next day his pulse was scarcely perceptible, and while I was in the ward he died.

Post-mortem Examination.—On examining the spine, no diseased appearance was found there, nor in the spinal marrow. The head appeared in a natural state, and the chest the same, but in the abdomen very great disease was found, and it must have been of long standing. Various parts of the cellular membrane under the peritoneum were found beset with innumerable granular bodies,—tubercles, as many persons would call them. The omentum appeared to contain a very large number of these, and in some parts it was very thick, on account of their deposition. The intestines were all glued together, and the omentum with them. On the right side of the abdomen, just between some parts of the intestines where complete adhesion had not taken place, a canal was formed half a foot in length. At the bottom of it the adhesions were still less, so that there was a great cavity or sack produced, but what this was on first opening the abdomen, it was impossible to say. On a more minute examination, however, by Mr. Nordblad, after the part had been removed, it appeared that there was not a real canal, but only a deficiency of adhesion between some portions of the intestines. All the rest of the intestine, the omentum, the parietes of the peritoneum, and the stomach, had been glued together into one mass, so that scarcely any abdominal cavity existed. The intestines thus adhered above, but not downwards, in consequence of which a canal was formed, and from the great want of adhesion at the bottom, there was also a large cavity. The canal was lined with a coagulum of blood exceedingly fine and thin, such as you see if blood escapes and is poured out over a large surface when it is fluid. The coagulum appeared in sheets, as it were. At the bottom of the large cavity to which the canal led, we found a quantity of blood partly fluid and partly in a state of coagulation,—a common coagulum of blood.

I do not know whether I can show you the parts very well now, but I will endeavour to do so. You observe that the omentum is very much thickened by a great deposit of granular substance between the layers of the peritoneum, of which you know the omentum is composed. If you look on each side of the omentum, you will observe the deposition between. Then you see under the peritoneal coat of the intestines the same granular deposition, and here between the two intestines, where the adhesion is not complete, there is a large cavity opening into a large sac at the bottom. This sac is not a new formation, but arises from the adhesion being imperfect. It really presents a very curious appearance; while all other portions of the viscera were glued together, these portions were not, and hence has arisen the large cavity, which of course we could make nothing of at the autopsy. Here is a portion of the peritoneum, and under it you see the deposition of this new substance. No doubt the deposition has taken place in the subserous cellular membrane. Morbid changes for the most part take place not in the serous membrane, but in the cellular substance underneath. I did not know at first whether this cavity was an aneurism, and it was impossible to know until the parts were minutely dissected. There are several tubercles bearing exactly the appearance that we see in the lungs. You now have the real state of the disease well enough shown. On examining the liver, under the peritoneal coat—not in it, but between the liver and the peritoneum that invests it, you find the same deposition. Here is the diaphragm adhering to the liver in every part, so that it was coherent. Here is the peritoneal coat, and here is the tubercular deposition. In order to illustrate the extent of the adhesion, I may show you the stomach, which you observe is adherent to the liver all around, forming one mass with it, and it adheres to the abdominal muscles in front, so that there was complete cohesion of every part. Here is the omentum adherent to the stomach. The gall-bladder is also adherent, so that you perceive the whole is united into one mass.

Remarks.—One cannot but wonder, in the first place, that such an immense mass of disease as this should have existed, and yet the man not make any complaint, nor any of the functions of the bowels be impaired. He was admitted on the 9th of February, and died on the 6th of March, and until just before his death he made no complaint whatever, except of uneasiness at the back of the neck, and sometimes down the spine. He ate well, slept well, and looked well. We saw that he was a slight, delicate, man, but he made no complaint of any sort, and his countenance, habit, and manner, led me to suppose that there was no complaint of any sort existing. As I have already stated, I found nothing the matter with his chest when I examined it, and the

disease of his abdomen appeared in itself of so slight a character, that I should not have discovered that he had this extensive disease at all, but being at a loss to explain some of the symptoms which did at last appear, and observing that he became emaciated and very weak, I was of course aware that there was some reason for what had taken place, and I determined, as I before said, to make a general examination of the body, and then I fixed upon the abdomen as the seat of the disease whatever it might be. Gentlemen who were present will recollect that I said at his bed-side, "Here is the disease which has occasioned this emaciation, and I presume, from the abdomen being rather enlarged, and hard, that we shall find disease of the mesenteric glands," and Mr. Nordblad tells me that those glands were in a state of disease. Here they are, and you see that they are not only diseased, but that some of them are actually in a state of suppuration, so that on squeezing them pus comes out. Some are very much enlarged, and in a state of scrofulous disease. Here is one particularly so. When this new set of symptoms appeared, accompanied by the debility, I gave the diagnosis that there was disease no doubt of the mesenteric glands, but I could not feel any organ enlarged. The liver and spleen did not appear to be enlarged, neither was there fluctuation. Therefore it was not effusion, and the man's bowels being regularly opened every day, and having indeed at that moment diarrhœa, it was evident that there was no accumulation in the intestines. It was that circumstance which led me to give the diagnosis which I have stated.

The reason why the disease existed so long without the patient suffering at all,—without his making any complaint, was that the internal surface of these organs was all sound. The inner surface of the stomach and intestines, as far as they were examined, and the substance of the liver, were all healthy. It was merely a disease of the outside of the different organs, and that is a sufficient explanation of the absence of symptoms, and of his general health not being materially affected.

But you will now ask me,—What was the reason of his suddenly becoming pale, emaciated, weak, and expiring so suddenly? I do not know that I can give you a satisfactory answer to this question; but when I consider that amidst those adhesions in the cavity which existed between them, a quantity of blood was found, I imagine that a degree of inflammation must have suddenly taken place along the sides of those portions which did not cohere – that a sudden degree of inflammation took place, attended with an effusion of blood. You saw a sheet of blood existing all down the false canal, and in the sac itself at the bottom was a quantity of coagulated blood. In all probability the change that took place in the midst of the adhesions caused an effusion of blood into the part, and this and the other morbid changes which occurred, were, I should think, from the examination that has been made, the cause to which we must attribute his sudden sinking. There is no suppuration to account for it; the mesenteric glands it is true are suppurated, but very slightly, and there is no part under the peritoneum in a state of suppuration, therefore it could not arise from this source. It is possible either that the changes which took place in this artificial cavity killed him, or the disease existing in the mesenteric glands, might have become so considerable that the chyle became completely obstructed, but whether that is the case or not I do not know, because it is rare even when there is great enlargement of the mesenteric glands, that you have an impediment to the course of the chyle.

The case is rendered very remarkable, from the long absence of the appearance of any ill effects, notwithstanding the disease must clearly have been of considerable duration. But I think that circumstance is sufficiently explained by the disease being situated outside the viscera. The reason of the sudden ill effects which were produced, I will not take upon me to explain very satisfactorily.

CASE OF CHRONIC RHEUMATISM, WITH OCCASIONAL ACUTE ATTACKS,

Treated alternately with Arsenic and Colchicum.—Immediate and ultimate Effects of Arsenic as a Medicine, &c.

The next case of which I will speak is one of chronic rheumatism, which was in William's Ward, and the man was there nine or ten months. It was a very interesting case on many accounts. His name was Samuel Bowman. He was thirty-nine years of age, and was admitted on the 2nd of June last, and then he had been ill three years.

Now his was a very severe form of rheumatism. It had rendered his hands nearly useless, as well as his feet. At the time of his admission his fingers were distorted; some were bent, and some were inflexible. He had very little power over them. Their figure was altered, and there was enlargement of different parts. It was a case of rheumatism which had produced considerable thickening of the ligaments, and more or less contraction of the tendons. His feet also were so much distorted that he could not walk. He was

neither better nor worse, he said, from different temperatures. Co'd did not make him worse, nor did it make him better, but he told me that his hands and feet were more frequently cold than hot. Their temperature was more frequently below what it ought to be than at the natural degree.

Treatment.—Under these circumstances I determined on giving him arsenic. In chronic rheumatism, where there is no morbid heat at all, nothing inflammatory to be discovered,—where heat does not increase the pain, and where you find the joints are stiff, arsenic is one of the best things you can employ. Now this man took arsenic from the 2nd of June up to the time of his presentation last week; that is, for nine months regularly, with the exception of certain intervals when acute rheumatism came on.

The case illustrates another fact common to all chronic diseases, viz., that notwithstanding the existence of a chronic affection, every now and then an acute attack supervenes upon it. It is very common in chronic affections of all kinds, for inflammation to take place at intervals, and the inflammation must be treated in the usual way, or you will do no good; but as there is a chronic affection existing, you must be on your guard, lest you should too much impair the patient's general strength.

Now this man several times during his stay here had a great degree of pain in his hands and feet, and this was rendered worse by the application of heat. He began to sweat, and his pulse became quick. These attacks were not confined to the parts affected with chronic rheumatism, but extending to the head, hips, shoulders, and various other parts of the body. Under these attacks I always left off the arsenic, and gave him colchicum. He took that medicine in the usual way till he was purged by it, and then, after he was purged, the dose of the medicine was diminished, and when the acute attack was got the better of, he resumed the arsenic again. These attacks occurred two or three times during the nine months that he was staying in the hospital. They were not at all referable to the arsenic; because they will occur in chronic rheumatism where persons are not taking that medicine, and not only in chronic rheumatism in general, but in that atonic kind which requires stimulants, and is attended by mere uneasiness of the part. As soon as the attacks were over he always resumed the arsenic, and under its exhibition his fingers gradually became more useful to him, and so did his feet, until at last he could walk with the assistance of another person. Such distortion, however, was produced, that he will never be able to use his hands or feet perfectly; that is out of the question, and he must remain a cripple more or less, no doubt, for life. But still his fingers and feet became more useful to him than before, his pains ceased. The size of the swollen part was diminished, and he acquired more power over them than he had when he came in.

Now this was a long time for a man to take arsenic, and I was obliged to omit it from time to time, not merely on account of the acute attacks of rheumatism, but also on account of its producing its common constitutional effects.

The effects of arsenic are twofold,—*immediately* upon the stomach, and *ultimately* upon the constitution; and it is the same with many other acrid medicines. When giving these acrid medicines, you must always be careful in watching for their effects. The local effects of arsenic are, heat in the region of the stomach—a sensation of burning and smarting there—tenderness on pressure, together with nausea and sometimes vomiting. Arsenic has a peculiar power of inducing gastritis, and the symptoms arising from inflammation of the stomach more or less active,—more or less in degree.

But all acrid medicines have the same effect. If you give nitrate of silver, you will observe the same. If you give iodine, you will observe the same, and so on with regard to other medicines of the class of acrids.

But independently of arsenic being acrid in the stomach, it has a specific power of inducing gastritis, if not enteritis. I have known this produced by arsenic when it was applied to a distant part of the body. Pains have come on at the pit of the stomach, tenderness on pressure, and vomiting, I will not say to the same degree, but in the same way that gastritis takes place if a person swallows arsenic as a poison, and, therefore, whenever you are exhibiting it, you should regularly at every visit (and you should see the patient pretty frequently) ask if there be any heat at the pit of the stomach, any heat up the throat, and you should feel whether there is any tenderness on pressure. It is right that you should ascertain this yourself. If it so happens that you cannot see the patient very often, you ought to tell him what effect the medicine may produce, so that he may be fully apprised of the period when it is exerting an effect which would be injurious. It is your duty, if you do not tell him what medicine he is taking, to inform him that he is taking something which may produce an unpleasant effect on the stomach; and you should tell him to press upon it, and see if there be pain.

But it also produces constitutional effects, which may make it necessary for you to

desist from exhibiting it. In this man those effects were very remarkable: in the first place it has the property of inducing inflammation of the eyes, and this man regularly after resuming the arsenic, and taking it for two, three, and four weeks, had inflammation of the eyes come on. This did not occur once or twice, but a dozen times during the period he was here. According to the dose he was taking, you saw that his eyes were red and filled with water. When I first saw that take place, I immediately omitted the medicine, but after it had occurred two or three times, I continued it for a few days longer, and did not leave it off until the inflammation became a little greater. As long as he said that his eyes were not sorer than at the last visit, I continued the medicine, but when he said they were getting sorer, then I omitted it. Arsenic, too, in many people, produces great heat, and possibly inflammation, of the throat, uvula, and soft palate; this is a very common circumstance: it will also induce heat of the fingers and redness.

It also occasions œdema of different parts. In this man this constitutional effect was produced, for his legs were swollen very frequently, or one arm was swelled, or his face. The face, particularly, is disposed to swell. This man's face was several times swelled, but at the last time his legs became œdematous. On leaving off the arsenic for a short time, the inflammation of the eyes always went down, and the œdema of the legs declined, and when he went out, were diminished very much.

The effect of the arsenic in this man was very satisfactory. It did what no other medicine would have done; but I should have thought it quite useless to have gone on with it, or to have kept him any longer in the hospital, because I conceive that the state of the parts was such as to be, in some measure, beyond the influence of medicine. But he went out much better, and was very grateful for the benefit he had received.

I do not know that arsenic is used in chronic rheumatism as much as it deserves. You will, however, find many writers recommend it in those particular forms of rheumatism where the parts are stiff and enlarged, and not in a state of heat, but, on the contrary, cold and relieved by heat, or, at any rate, where a hot temperature is not uncomfortable. You will find that Dr. Bardsley of Manchester, father I believe of the present gentleman of that name, published in its favour; and in the *Edinburgh Journal* there are some papers by Mr. Hill, also recommendatory of it. It was from the perusal of those papers that I first thought it would be worth while to employ it in this particular complaint. In acute rheumatism it does no good, nor will it do good in rheumatism of a decidedly inflammatory character; and, on the other hand, I never saw colchicum do good in rheumatism, when the parts were not hotter than they should be. Now and then it certainly may appear to do good, because sometimes the most useless medicines will appear to be beneficial, the patient in fact getting well in spite of all the bad practice that is adopted. But I never saw anything that would lead me to imagine that colchicum did good in rheumatism that was not attended by morbid heat of the parts, or in which an increased temperature did not do harm. When the parts are hotter than they should be, or heat increases the pain, then I see colchicum do good; but when the parts are cold, I never saw any benefit arise from it. When, however, there is stiffness of the joints, especially of the smaller joints, and coldness, and when warmth relieves, then I believe arsenic to be a very excellent medicine. The dose may be from 3 or 4 to 12 or 14 minims three times a-day.

Curious Case of Poisoning with Arsenic.

It is this peculiar effect of arsenic on the eyes which made me suspect the other day that a whole family had been poisoned with arsenic. I was consulted on a very curious set of cases. A whole family was seized with nausea and vomiting, and every one of them had watery eyes. This continued sometime; I found all the maid servants with watery eyes, and that they had had nausea and vomiting. There were four or five children also in the same state, as also was the lady of the house. The husband happened to be out of town. None of them had a pulse under 120, and in some it was as high as 160. The children were all going about the house, and the servants were engaged at their work. The man servant as well as the maids all had rapid pulses with watery eyes. On looking at their tongues, I found they were red and foul, and they had also had thirst, and a sensation of heat at the stomach. I endeavoured to ascertain whether persons in the neighbourhood were in a similar condition, but that was not the case, not even with their nearest neighbours, and therefore I presumed the affection was not owing to the atmosphere. Then respecting the ingesta, I could discover nothing that was at all capable of explaining the symptoms. I found that they all had the general heat of the body which I have mentioned, as well as of the stomach and eyes; in fact a general inflammatory state.

Seeing these things, and finding that the watery state of the eyes and the nausea had been going on day after day, I suspected that they were under the influence of arsenic. When arsenic produces those constitutional

effects which it is necessary to remedy, you find bleeding by far the best method that can be adopted. When arsenic has been taken into the stomach, you endeavour to get it out as well as you can, but after that it is necessary to bleed on account of the gastritis, and the inflammatory state of the system. When you have got it all out of the stomach of the patient, there may still be the greatest danger, not only from the inflammatory state of the stomach, but the general inflammatory state of the body; the great remedy for this is bleeding. I felt convinced that this family must have suffered from arsenic, and they were bled, and some proportionately relieved.

The water which they drank was examined by several eminent chemists, and no arsenic could be found in it, but the persons who had lived in the house before had been mixers of colours, and some of their combinations contained arsenic, and before they quitted the house, it turned out that they had deposited in the kitchen, and in the garden all around the house, a great quantity of arsenite of copper. I did not learn this at first, and I could not imagine where the arsenic came from, but when the husband returned home and stated this, on relating the case to an eminent chemist, he told us that he was satisfied that water coming to the arsenite of copper would produce arseniuretted hydrogen, which was let loose, and produced the effect on the family. He considered that the arsenite of copper was calculated to become pernicious by water being mixed with it. The situation of the house was damp, and water had free access to the arsenic contained in the colouring matter, which had been deposited only last summer under the kitchen and every part of the house. Several barrows full of this stuff were removed. It was an eminent chemist who informed us that, under all the circumstances, he had no doubt that arseniuretted hydrogen had been disengaged, and I had felt satisfied, from the effects I saw, that arsenic must have been the cause of all the symptoms. No medicine was given, nor was anything of the kind required, as the arsenic was not in the stomach, but had pervaded the whole system. It was altogether an extraordinary occurrence, and I never witnessed anything of the kind before. The whole family were affected. All had a pulse from 120 to 160, and the whole of them were affected with a running of the eyes, but they all got perfectly well, except that pain of the limbs came on after they had appeared well for some time.

ST. GEORGE'S HOSPITAL.

CLINICAL LECTURE

DELIVERED BY

Dr. SEYMOUR,

April 10, 1832.

HYSTERIA.

Gentlemen,—There were some cases of hysteria dismissed on Wednesday last. Of two of these, however, I shall speak to you at some length. One of them was a common case, such as is to be met with every day. The other was a severe one. In the treatment of hysteria, it is highly important that you should distinguish real cases from feigned ones. On this particular point you may read what Sydenham says with great advantage, for the observations which he makes will apply as well to this disease in the present day as in the days in which he wrote. Hysterical symptoms have generally some sympathetic reference to the uterus. In hysteria, as we generally see it, the patient has a very great disposition to laugh or to cry, with a peculiar sensation in the throat, as of a ball rising in it. This latter symptom is entirely a nervous affection. In some cases the patient has spasmodic twitchings, and these constitute what is commonly termed an *hysterical fit*; but there may be hysteria without this. It is, in general, caused by some violent emotion of the mind. You find it as common in the higher as in the lower classes, but with this difference, that among the latter it frequently arises from painful impressions on the mind, or from being closely confined within doors, bad air, &c. In weak persons you have generally an irregular action of the uterus. In some cases you will find pain over the eye denominated *clavus hystericus*. Sometimes you will find pain in the left side over the spleen, or great curvature of the stomach. Sometimes on the other side you will find the patient complain of pain over the region of the liver, accompanied with great flatulence, the discharge of which always relieves the patient. This is generally pent up either in the stomach or colon. Sometimes you will find the patient complain of most excessive pain. Depend upon it this is hysteria, for the excessive pain is here diagnostic of the complaint. If there were inflammation to cause this excessive pain, the pulse would show it. The rising in the throat, the pain over the eye, and the flatulency, are common symptoms of the affection. In some cases, however, the symp-

toms are worse, and the patient loses the use of his lower limbs, and there is paraplegia and sometimes hemiplegia, but these symptoms are in general cured. Sometimes you find the pulse slow and feeble, and the ankles puffed. If the attack of hysteria be acute, it is most difficult to distinguish. In the pain in the throat you will sometimes be led to consider it as inflammation of the throat, and if you give diffusive stimulants you cure it. I have been frequently disappointed in cases of this kind occurring in the hospital.

(Dr. Seymour here related the particular symptoms of the cases on which he founded these observations; but the only point in them to which we need allude, was that of blisters being applied to expel wind.)

These cases you will perceive, Gentlemen, are very simple ones; but you must learn to treat such as these before you go to more complex ones. The pain in the spleen in this disease is a very common symptom. Hence the term of *spleen*, or *vapours*. Dr. Rowley, who was predecessor to Dr. Hooper at the Marylebone Dispensary, wrote a book stating hysteria to be a disease of the spleen. Patients afflicted with this disease seldom die of it. Of the pathology of the disease I cannot tell you anything, for there is none; therefore, to say that the disease is one of the spleen, a viscus of the pathology of which we know so little, is to endeavour to remove one difficulty by setting up another.

(The notes of a severe case were here read, on which Dr. Seymour continued,)

I never saw so much improvement in any case as in this, on the catamenial secretion being restored after it had been lost for eleven months. The disease was here more functional than organic. Paraplegia and hemiplegia, you know, are in general caused by organic disease of the brain, and they will not go off until the organic disease be removed; but when these symptoms go off and return again as suddenly, they cannot arise from the presence of organic disease, but from hysteria. In my case-book you will find that leeches were ordered to be applied behind the ears, but they were really applied to the groins. In these cases you must always respect the feelings of your patient, and it was for this that I ordered the leeches to be applied behind the ears. Many gentlemen practising midwifery in London (and the late Dr. Gooch was also of their opinion), have informed me that the application of leeches to the groin will reproduce the catamenial secretion. I have never found this to be the case, and in the present instance the leeches did not restore the catameniæ. Sydenham says, that one half of the diseases of females are dependent upon hysteria. There is no disease in which you will meet with more imposition than in this, and that too in cases where the patient's rank in life, her station, and education, would all lead you to believe such imposition on her part to be quite impossible. Females will sometimes insist upon it that they have a disease. I once heard of a case in which a young lady was said not to have passed a drop of urine for six weeks. She had diuretics and every other stimulant to the urinary organs, but all to no purpose. Her medical man now began to suspect the case was all trick; she was narrowly watched, and it was discovered that she *drank her urine*. I was once shown a very large quantity of gravel which a young lady passed with her urine. It was reported to me that she had violent pains in the back and loins. She had gone through the whole list of medicines usually prescribed for these cases, and by way of doing something, she had been sent to the sea-side. Now I do assure you, that the gravel which was shown me was nothing more than sea-sand. The above are cases of downright imposition, and in all such cases as *these*, I would advise you to let the patient and her friends know that you are fully aware that her disease is all imposture, and you will generally find this remark sufficient to cure the worst symptoms.

But there are other cases of mixed ones, in which the patient may perhaps be ill; but she will lay very great stress upon it; nothing will relieve her; whatever medicine she takes only tends to make her ten times worse than she is; purgatives confine her bowels; opiates do not make her sleep; diuretics stop the secretion of urine. If you give her medicines for any particular pain, they will only aggravate it tenfold, and so forth. The pains of which she complains are referable to no known disease; indeed she has pain everywhere. Now all this I need not tell you arises from excessive nervous sympathy, and as such it must be treated. Your remedies must always have a tendency, if only in the patient's imagination, to remove her complaint; and you must change, renew, and alter them, according to the Protean shape which the disease assumes. You will sometimes find the simplest remedies cure, where all others have failed. Belladonna rubbed on the back of the neck will sometimes cure. Doing nothing at all will, in fact, more frequently prove your best remedy, after surgeons and physicians without number have been applied to in vain.

You will at once perceive that all these complaints depend upon excessive irritability of the nervous temperament; and anything that will powerfully excite the imagination of the patient in this suscep-

tible state, will effect a cure. It is in such cases as these that non-educated practitioners, quacks, and charlatans, obtain such great credit to the detriment of our profession. It is in similar cases to these that Prince Hohenloe worked such pretended miraculous cures. As you may well imagine, curative medicines of all kinds have been tried, and in vain. Moxa, cautery, blistering, and every other counter-irritant of that class, have been tried. But above all, in your behaviour to your patient you must never at first let her see that you undervalue her complaint, or disbelieve the statements she makes to you of it, however improbable or unlikely they may appear. If you do, she will only endeavour to deceive you still more, and eventually decline seeing you again. I knew a patient who used to be violently sick every morning, and nothing she took would stop the vomiting, until it was discovered that she took opium every night to bring on the sickness in the morning. In all these, and I may say in every case of hysteria, your best remedies are perseverance, patience, and care. These will do more for you than anything else. I knew of the case of a young lady who could not swallow. The first surgeons and physicians in London were applied to for their advice, as well as quacks without number, but all failed. At last, for want of a better remedy, cold affusion was tried, and this proved successful. You must exercise your ingenuity in varying your remedies every time you see your patient; the cold-bath; the warm-bath; change of air; the country; the sea-side; residence in a foreign climate. Sometimes one of these will cure a patient, or she will fancy she is cured; or if you tell her she has a new disease altogether, and prescribe a plan of treatment such as she has never heard of before, she will frequently get well without anything else. If you find the patient low and weak, you had better give diffusible stimuli, as sometimes, in such cases, you will find your patient will have fits. I knew a young lady who lived upon three tablespoonfuls of tea daily. She could not, or she would not, take anything else; but by diverting her mind, and gradually habituating her to a new plan of treatment, she got quite well. In the course of your professional practice, you will meet with innumerable varieties of this disease. You will hear patients say that they pass calculi from the bladder, and when you come to examine them, they will prove to be common flint stones. But you must have patience with all these cases. If you should meet with a case of imposture, never shrink from telling the patient or her friends that you know she is imposing upon you, for no professional man should lend his time and his trouble to a case of imposition.

But, as Sydenham very truly remarks, it would take a whole day for me to discuss every shape, form, and feature, in which this disease will present itself to you. I have, however, detailed to you most of the ordinary symptoms which you will be liable to meet with, and I hope the account of them will be the means of guiding you in some measure towards a correct treatment of this changeable disease.

GASTRODYNIA.

In the treatment of gastrodynia you will find the nitrate of bismuth of great service. It acts as an absorbent, and relieves the spasmodic affection of the stomach. I have had a case of this disease under my care in this hospital, in which however I found rhubarb and magnesia of very great service, under which the patient got materially better. Gastrodynia is generally caused by the presence of acidity in the stomach, and the patient should desist from vegetable acids of all kinds, as they weaken the stomach. You know that the gastric juice of the stomach is acid, and this acidity is increased when gastrodynia is present. Nevertheless, in some cases you will find the most obstinate and formidable symptoms cured by the simplest remedies. There was a case in this hospital, some years ago, of severe vomiting from the stomach, and it was supposed to arise from cancer of the pylorus. All the great guns were fired off on the occasion; every remedy was tried; conium, blistering, &c., but all without any good effect. Dr. Pearson, who had the patient under his care, was not at all alarmed at all these symptoms. He gave the calcined magnesia for a short time, and this effectually cured this terrible case of "cancer of the pylorus." This will show to you how a violent disease may be cured by the simplest remedies, and by those that are most agreeable to the feelings of the patient.

LIGATURE OF THE PENIS.—A Jew-boy four weeks old, was suckled by a nurse whom the family became dissatisfied with, and dismissed. Two days afterwards Dr. G. was sent for on account of painful swelling of the boy's penis, when he found a long hair five times wound and tied round the root of the penis. With great difficulty the hair was removed from the deep incision it had made, which emollient applications speedily healed; but had it remained a short time longer, gangrene would inevitably have occurred. "Very probably," says Dr. G., "the nurse wished in this manner to revenge herself for her dismissal."—*Russ Mag.*, 1831.

LECTURES
ON
VETERINARY MEDICINE,
DELIVERED IN THE
UNIVERSITY OF LONDON,
BY
MR. YOUATT.

Lecture XXVIII.

THE SYMPTOMS, CAUSES, AND TREATMENT, OF INFLAMMATION OF THE LUNGS IN THE HORSE, CATTLE, SWINE, AND DOGS.

We will now proceed from the mucous membrane of the bronchial passages to that which lines the air-cells of the lungs, or to the proper cellular texture or substance of the lungs,—the interior of the lobuli to which we have traced the air-tubes, and in which the important change of the blood is effected. I have said, that the intimate structure of these lobuli has never yet been demonstrated; but we can scarcely avoid considering them as composed of little cells or pouches, into which the air is at length conducted, and over the delicate membrane constituting the divisions of which myriads of minute blood-vessels are ramifying. The blood is not merely permeating them, but it is undergoing a vital change in them: there is a constant decomposition of the air, or of the blood or of both, and during the excitement of exercise, that decomposition proceeds with fearful rapidity. Then we can readily conceive that a membrane so delicate as this must be, in order that its interposition shall be no hinderance to the arterialisation of the blood, so fragile also, and so loaded with blood-vessels, must be exceedingly subject to inflammation, and that of a most dangerous character.

Causes.—Inflammation of the parenchyma of the lungs is the not unfrequent consequence of all the diseases of the respiratory passages that we have considered. Catarrh, influenza, bronchitis, if neglected or badly treated, or sometimes in spite of the most skilful treatment, will spread along the mucous membrane, and at length involve the termination of the air-passages. At other times we have pure pneumonia. This cellular texture is the primary seat of inflammation. It is often so in the overworked horse. After a long and hard day's hunt, it is very common for horses to be attacked by pure pneumonia. A prodigiously increased quantity of blood is hurried through these little vessels, for the vast expenditure of arterial blood in rapid progression must be provided for. These minutest of capillaries are sadly distended—irritated; their contractile power is destroyed—inflammation is produced—mechanical injury is effected—the vessels are ruptured—blood is poured into the interstitial texture, and this increases the irritation,—and inflammation and congestion, with all their train of fatal consequences, ensue.

Alternations of Heat and Cold.—These are the most frequent causes of pneumonia. A sudden change from heat to cold,—the change from a warm stable to a colder one—the neglect of the usual clothing—the neglect even of some little comforts—riding far and fast against a cold wind, especially in snowy weather—loitering about when unusual perspiration has been excited—loitering tediously by the side of a covert on a chilly blowing morning:—the experience of every day convinces us, that these are the most frequent causes of pneumonia. The thing is so plain, that I am almost ashamed to detain you for a moment about it. Can any one hesitate as to the effect of cold applied to the extensive surface of the skin—closing so many pores—determining so great a quantity of blood to other parts—causing disturbance every-where, and especially in the pulmonary circulation,—in that part which, from the delicacy and complexity of its organisation, and more especially from the injury it has received by our absurd and cruel management of the horse, is most of all subject to inflammation, and that of the intensest kind?

It has not unfrequently happened, that when horses have been turned out too early to grass, or without gradual preparation, pneumonia has supervened, and especially if other causes have assisted. Few horses even in their stable-state, are so subject to pneumonia, as they who, in poor condition and without preparation, are turned into a salt-marsh. The Plumstead marshes below Woolwich are notorious for this.

On the other hand I can imagine, that a sudden and considerable change from cold to heat, shall be followed by inflammation of the lungs. Many horses perish in the dealers' stables from this cause. The circulation is considerably quickened; more blood, and that more rapidly, is driven through the lungs, previously disposed to take on inflammatory action; yet even here the sudden removal from the heated stable to the cold air for the purpose of examination, has much to do with the production of disease. But enough of the causes of pneumonia.

Symptoms.—Whether it is the consequence of previous disease of the respiratory passages, or inflammation first appears in the cellular texture of the lungs, pneumonia is usually ushered in by *rigor*. The horse is cold all over; this, however, soon passes

over, and we have general warmth, or heat of the skin above the usual temperature, but accompanied by *coldness of the extremities*—intense deathy coldness. This is a perfectly diagnostic symptom: it will never deceive you It is an early symptom; it is found when there is little or no constitutional disturbance,—when the pulse is scarcely affected, and the flanks heave not at all, but the horse is merely dull and off his feed. It is that by which you will mark the progress of the disease when others scarcely suspected its existence. The blood is determined to the inflamed part—it is congested in the lungs—a diminished portion of it passes the left side of the heart—it scarcely reaches the extremities, or it is not sufficiently arterialised, and little heat is consequently extricated in its change to venous blood.

The *pulse* is *not* always at first *much increased* in rapidity, and it is rarely or never hard: it is *obscure, oppressed*. The heart is labouring to accomplish its object—the circulation through the lungs is impeded—the vessels are engorged, they are ruptured—blood is extravasated into the air-cells—it accumulates in the right side of the heart—and in the larger vessels and in the venous circulation generally, there is a mechanical obstruction, which the heart has not power to overcome; thence the obscure oppressed pulse,—the ineffectual attempt to urge on the blood, and hence, too, the remarkable result of bleeding in inflammation of the lungs;—the pulse becomes rounder, fuller, quicker. When blood is abstracted, a portion of the opposing force is removed, and the heart, now evidently accomplishing its object, the pulse is developed.

It is only, however, in the early insidious stage that the flanks are occasionally quiet; if the compressibility of the lungs is diminished by the thickening of the membrane, the engorgement of the vessels and the filling of the cells, it will be harder work to force the air out; there must be more evident effort, and that which cannot be accomplished by one effort is attempted over and over again. *The respiration is quickened—laborious*; the *inspiration is lengthened*, to lessen the pain which the distension of the vessels occasions; the *expiration is rapid*, to get rid of the pain caused by the state of distension; and when, after all, the lungs cannot be compressed by the usual means, every muscle that can be brought to bear upon the part, is called into action; hence *the horse will not lie down*. He can use the muscles of the spine and the shoulder with most advantage, as he stands: hence, too, the very peculiar stiffness of position—the *disinclination to move*. There are various muscles whose action can be materially changed when we change the fixed point. The muscles which compose the serratus magnus influence the motion of the shoulder, while the chest remains a fixed point; but when by throwing the whole weight on the fore extremities the limbs are made the fixed point, the chest is materially expanded by the action of the same muscles. Therefore the horse with decided pneumonia can scarcely be induced to move at all; he cannot spare for a moment the assistance which he derives from these muscles; he will continue obstinately to stand until he falls exhausted or dying. How eagerly do we ask when we go into the stable, Was he down last night? And we conclude that we have not made much progress in the case when the answer is in the negative. When he is wearied out and lies down, it is only for a moment. If the inflammation is not subdued, he cannot dispense with the auxiliary muscles. He frequently, and with doleful expression, *looks at his sides*—at one side or at both, accordingly as one or both are involved; there is not, however, the decidedly haggard countenance of bronchitis, and you will recollect, that in bronchitis the horse rarely or never gazes at his flanks,—his is a dread of suffocation more than a feeling of pain. The *head is protruded* and *the nostrils distended*, and *the mouth and the breath intensely hot*. *The nose is injected* from the earliest period, and there is not merely injection, but the membrane is uniformly and intensely red. The variation in this intensity is anxiously marked by the observant practitioner, and he regards with fear and with despair the livid or dirty brownish hue that gradually creeps on. The continued intensity of inflammation he can probably combat, but he cannot repair the exhausted and disorganised tissue.

Progress of the Disease.—The unfavourable symptoms are, increased coldness of the ears and feet if that be possible, partial sweats, grinding of the teeth, evident weakness, staggering, yet the animal will not lie down; pulse quickening, and becoming weak and fluttering; the membrane of the nose paler, but it is a dirty paleness; the animal becoming stupid, comatose: at length he falls, but he rises immediately; for awhile he is up and down, until he is no longer able to rise; he then struggles severely—groans; the pulse becomes more rapid, fainter, and he dies of suffocation. The disease sometimes runs its course with strange rapidity. I have known a horse destroyed by pure pneumonia in twelve hours. The vessels ramifying over the cells have yielded to the fearful impulse of the blood, and the lungs have presented one mass of congestion.

The favourable symptoms are; the return of a little natural warmth to the extremities;

the circulation beginning again to assume its natural character; and, next to this, the lying down quietly and without uneasiness; showing us that he is beginning to do without the auxiliary muscles. These are good symptoms, and they, too, will rarely deceive.

Terminations of Pneumonia.—*Congestion* is a frequent termination of pneumonia. Not only are the vessels gorged,—the congestion which accompanies common inflammation, but their parietes are necessarily so thin, in order that the change in the blood may take place, although they are interposed, that they are easily ruptured, and the cells are filled with blood. This effused blood soon coagulates, and the lung when cut into presents a black, softened, pulpy kind of appearance, termed, by the farrier and the groom, *rottenness*, and being supposed by them to indicate an old disease. It proves the violence of the disease, the rupture of many a vessel surcharged with blood; and it also proves that the disease is of recent date; for in no great length of time the serous portion of the blood becomes absorbed, the more solid one becomes organised, the cells are obliterated, and the lung becomes *hepatised*, or bears considerable resemblance to liver. The farrier, is now, however, becoming a little enlightened on this point, and regards this appearance of the lung as indicating intense but recent disease; yet only a few years ago many a dealer was unjustly punished in his pocket for selling a rotten horse, whereas the inflammation and effusion had been produced by some accidental cause or by too early and severe work, after the animal had left the dealer's stable, in which he had been made up for sale.. Here is a very fine specimen of congestion. The cells appear to be all filled; the lung is quite impervious; respiration could no longer be carried on, and the horse died of suffocation. Here is another specimen in which the inflammation appeared also to pervade the whole of the lung; there is a redness; great turgidity of the vessels every-where, but it has not run on to effusion; the vessels were not ruptured.

The Value of Auscultation.—In every case of pneumonia, have early and anxious recourse to *auscultation*. Make yourselves perfectly acquainted with the deep distant murmur presented by the healthy lung. This sound is most distinct in the young horse, and especially if he is a little out of condition. On such a horse the tyro should commence his study of the exploration of the chest. There he will make himself best acquainted with the respiratory murmur in its full state of development. He will next take an older and somewhat fatter horse; he will there recognise the same sound, but fainter, more *distant*. In older and fatter horses there will sometimes be a little difficulty in detecting it at all. Repeated experiments of this kind will gradually teach the young veterinarian what kind of healthy murmur he should expect from every kind of horse that is presented to him, and thus he will be better enabled to appreciate the different sound exhibited under disease.

Attention to Auscultation urged.—Before Laennec had led the human practitioner to adopt the invaluable method of exploration, or the veterinarian had dreamed of such a mode of examination, the most useful guide we had in ascertaining the nature and progress of disease in our dumb patients was the pulse. I think I can safely say that for nearly a twelvemonth I daily studied the pulse of every horse in the Veterinary College, anxiously comparing it with the age, breed, disease, and varying character of disease; and I look back on these hours as spent the most usefully of all in the preparation for professional duty. You, Gentlemen, have now another guide fully as valuable in all the diseases of the chest. Avail yourselves of the same opportunity to derive from it all the advantage it can afford you. It will be time most usefully spent, and the recollection of it will be gratifying to you. In this perambulation of the hospital, and in the cultivation of morbid anatomy—not a succession of barbarous operations and experiments; do not disgrace yourselves by them—they will not afford you matter for pleasing reflection hereafter;—in the cultivation of morbid anatomy I say in the knacker's yard, you will be laying a surer foundation for future reputation and usefulness, than in the attendance on any lectures, however gifted the teacher.

The Crepitus of Pneumonia.—Then make yourselves acquainted with the deep distant murmur presented by the healthy lung. If pneumonia exists to any considerable degree, this murmur will be changed for, or mingled with, as I observed in the last lecture, a curious *crepitating sound*. This once remarked cannot well be mistaken. It is caused by the infiltration of blood into the air-cells; its loudness and perfect character will represent the intensity of the disease, and the portion of the chest at which it can be distinguished will clearly indicate the extent of the disease.

Hepatisation.—The whole lung, however, is not always affected, or there are only portions or patches of it in which the inflammation is so intense as to produce this congestion; enough remains either unaffected or yet pervious for the function of respiration to be performed, and the animal lingers on or perhaps recovers. Then the process which I have just described will

take place where the inflammation has been most intense. The serous portion of the blood will be absorbed, and the more solid will become organised, impervious, *hepatised*. By careful examination with the ear, you will be enabled to get at this also. Where the lung is impervious—where no air passes, no sound will be heard, not even the natural murmur. Around it the murmur will be heard, and louder; it will be a kind of rushing sound; for the same quantity of blood must be arterialised, and the air must rush more rapidly and forcibly through the remaining tubes: and if there be considerable inflammation or tendency to congestion, the crepitating crackling sound will be recognised, and in proportion to the intensity of the inflammation. Gentlemen, I do not overrate the advantages to be derived from the study of auscultation. It was strong language lately applied by an able critic to the use of the *stethoscope* by the human practitioner; I would literally apply it to the use of immediate auscultation, that "it converts the organ of hearing into an organ of vision, enabling the listener to observe with the clearness of ocular demonstration the ravages which disease occasionally commits in the very centre of the ribbed-cased cavity of the body."

A horse with any portion of the lungs hepatised cannot be *sound*. He cannot be capable of continued extra-exertion; his imperfect and mutilated lung cannot supply the arterialised blood which rapid progression long continued requires, and that portion which is compelled to do the work of the whole lung must be exposed to injury and inflammation from many a cause that would otherwise be harmless. Here are several specimens of hepatisation.

Tubercles.—Another consequence of inflammation of the substance of the lungs is the formation of *tubercles*. There is a great deal of mystery, and there has been some acrimonious dispute, about this. It would seem that at first there are a greater or smaller number of little distinct cysts, cells, into which some fluid is poured in the progress of inflammation; the cells become gorged, distended, and occupy a space varying from a pin's point to a large egg. Here is a beautiful specimen of tubercles of the lung, each nearly the size of a filbert. Here is another in which they are more numerous, but not so large as the smallest millet-seed. This last belonged to a glandered horse.

By degrees the fluid becomes concrete; the little tumour is much harder than the surrounding substance, and so it continues for a while—the consequence and the source of inflammation. It occupies a space that should be employed in the function of respiration, and by its pressure it irritates the neighbouring parts, and exposes them to inflammation.

By-and-by commences another process never sufficiently explained. The tubercle begins to soften at its centre,—a process of suppuration commences, and proceeds until the contents of the cyst become again fluid, but of a different character—now consisting of pus. The pus increases; the cyst becomes more and more distended; it encroaches on the substance of the lungs on every side; it comes into contact with other tubercles, and the walls opposed to each other are absorbed by their mutual pressure; they run together and form one cyst;—morbid anatomists term this a vomica. It is no process of ulceration, no corroding or eating into the substance of the lungs, but a gradually-increasing and regular excavation, and this sometimes runs on to a very great extent, until a considerable portion of the lung is, as it were, hollowed out. By-and-by however the vomica presses upon some bronchial passage, and the cyst gives way; and the purulent contents are poured into the bronchiæ, and got rid of by the act of coughing—a great quantity of pus and often fœtid is thrown up—at other times the quantity is too great to be thus got rid of, and the animal is suffocated. Sometimes it will break through the pleuritic covering of the lung, and pour its contents into the thorax.

Abscesses may exist in the Lungs undiscovered.—It is scarcely conceivable to what extent these abscesses may exist in animals of slow work, without their being discovered by the usual means of examination. Mr. Hales tell us that he gave a physic ball to a cart-mare with bad foot, and she soon afterwards died suddenly. When inquiring as to the cause of death, he was told, and not very good-humouredly, that his physic had killed her. He asked if it had purged her violently? "No!" it was replied, "it had not operated at all." She was opened, and the mystery was all unravelled. The thorax was deluged with pus, and there were then in the lungs several large abscesses, one of which contained at least a quart of pus. The mare had not shown a symptom of chest affection, and the gentleman to whom she belonged, declared that he had believed her to be as sound as any horse he had in his possession.

Sometimes I believe there is true ulceration of the lungs, and seemingly not connected with the presence of tubercles. I cannot tell you how you may ascertain this. I am not sufficiently master of auscultation to describe the difference in the sound, or, rather, as no sound would be given immediately from the part through which no air passed, it would be impossible to distinguish the one from the other. One thing

however we know, that where the progress of ulceration is rapid the horse must die—his wind might be impaired, but his life would not always be endangered by the existence of tubercles.

Resolution of Inflammation.—The resolution or gradual abatement of inflammation is the termination most to be desired, and then the engorgement of the vessels will gradually cease, and the thickening of the membrane and the interstitial deposit will be taken up, and the effusion into the cells will likewise be absorbed, and the lungs will gradually resume their former cellular texture; yet not perfectly: there will be some induration, slight but general; or some more perfect induration of certain parts, or the rupture of some of the air-cells, or an irritability of membrane predisposing to renewed inflammation. We may save the horse, but he will not always be as useful as before. These sequelæ, under the terms chronic cough, thick wind, broken wind, merit distinct consideration, but for the present I must proceed to the treatment of pneumonia.

Treatment—Bleeding.—Here again commence with bleeding. You have inflammation of that organ through which all the blood in the frame passes—that organ most of all subject to congestion. Nothing can be so important as to lessen the quantity of blood which the heart is endeavouring to force through the minute vessels of the lungs, distended, irritated, breaking. When you bleed in other cases you lessen the quantity of blood given to the part only in proportion, and that often a very small one, to its share of the general mass of fluid; but here every drop you take away is so much subtracted from the quantity of blood permeating the overdistended and overworked capillaries. Then bleed, and bleed copiously too. In a decided case of inflammation of the lungs, try to knock the disease down at once—it is your best, and sometimes will be your only chance. Take, in a manner, as much blood as you can get: let the stream flow on until the pulse falters, and the animal bears heavy upon the pail: and take the blood which you do take as quickly as you can take it—let your lancet be broad-shouldered, and the orifice large. This is the secret of treating inflammation of a vital organ, and of an organ like this. You weaken or destroy the disease, without permanently impairing the strength of the patient; whereas by small bleedings, and with a small stream, you are sapping the strength of the patient, while the disease remains untouched. Gentlemen, if I can only impress this truth upon your minds,—this polar star in practice—the memory of our transient connexion will not be displeasing—to either of us.

Physic.—Next comes purging—yes—if we dared; for we should get rid of some cause of excitement, and lessen the circulating fluid, and produce a new determination of the vital current: but experience teaches us that in pneumonia there is so much sympathy with the abdominal viscera,—there is such a fatal tendency in the inflammation to spread over every mucous membrane, that purging is almost to a certainty followed by inflammation, and that inflammation bids defiance to every attempt to arrest it. I speak with perfect consideration, when I affirm that, in the majority of cases, a physic ball would be a dose of poison to a horse labouring under pneumonia.

May I not relax the bowels? Yes, if I could stop there. In my own infirmary, and there we should endeavour to get all cases of this description, I may, after the inflammation has evidently a little subsided, venture upon, yet very cautiously, small doses of aloes—half a drachm two or three times in the day in my fever medicine, and I may quicken it by frequent injections of warm soap and water; omitting my purgative however the moment the fæces are becoming pultaceous: but I must be assured that the inflammation is subsiding, and I must have considerable constipation, or I had better let my purgative alone. I have, in an early period of practice, more than once killed a horse labouring under inflammation of the lungs with two drachms of aloes.

Sedatives.—If I must not give physic I must endeavour to find some auxiliary to the bleeding, and I have it in the compound of *digitalis, nitre, and emetic tartar*, that I have so often recommended. The foxglove, a direct and powerful sedative, whose principal effect is produced on the heart, lessening the number of pulsations, and that by a curious intermission of the heart's action; while its secondary effect is also a useful one—namely, that of a diuretic. Some practitioners prefer the *white hellebore*. It is a powerful nauseant—it lowers the frequency and the strength of the pulse, but it does so almost on the principle of a blister, by producing a powerful counter-determination of blood, and that principally to the brain. By the excitement of the brain or its membranes, which it seems to produce, it diverts the inflammation, or a portion of it, from its original seat. In this respect it is as powerful as we could wish, and in our own infirmary, and where we see our patient a dozen times in the day, it may be used with safety: but, out of doors, and where we do not see him more than once in twelve or four-and-twenty hours, it seems a little too much like playing with edged tools. If the tremor, the vertigo, and the purging, which

it produces, are carried a little too far, the patient may be lost. Generally speaking the hanging of the head, and the foaming at the mouth, and the sinking of the pulse, may give warning; but our assistant may not observe these symptoms, or they may have existed many an hour before he arrives. In our own hospital it may be admitted, and will not often deceive, but even there we could wish for something as effectual and safer, and you recollect the account in the last lecture of its alarming effects when administered in a case of bronchitis. We however have that which is even more effectual, and which never did harm, the digitalis. In the course of twenty years' practice I do not recollect a single case in which I had cause to repent of the exhibition of digitalis, but I have attributed the deaths of more patients than one to the incautious use of hellebore. The nitre is a refrigerant and a diuretic, and the emetic tartar is a nauseant and a diaphoretic.

General Care.—Having determined on the medicine, I think of the kind of care which the patient needs. I order his legs to be well hand-rubbed, in order to restore if I can the circulation to the extremities; and I direct that comfortable flannel rollers shall encase the legs from the foot to the knee. I cover him up warm. Can there be a doubt about this? I wish to divert the current of blood as much as I can to the skin. I know that cold applied to the extremities will determine the blood to the centre, and cause it still more injuriously to congest upon and around the seat of inflammation; therefore I uniformly clothe him well. As for air, in warm weather, he cannot have too much. In cold weather his box must be airy, but not chilly. I want to determine the blood to the extremities and the skin, but not all the clothing in the world will keep my patient warm, if he is placed in a cold and uncomfortable situation.

Food.—As for food, I think not of it. In nine cases out of ten he will not touch anything, and if he is inclined to eat I give him nothing but a bran-mash, or a little green-meat, or a few carrots.

Blisters.—I now look about me for some counter-irritant. I wish to excite some powerful action in another part of the frame, and which shall divert the current of blood from the part first affected. I recognise it as a law of nature, and of which I here eagerly avail myself, that if I have a morbid action in some vital part, an unusual determination of blood to that part, I can abate, perhaps I can at once arrest, that morbid action by exciting a similar or a greater disturbance in some contiguous and not dangerous part. Therefore I blister the sides and the brisket; I produce all the irritation I can on the integument, and in proportion as I do so, I abate the inflammation within. But then there are certain considerations which I must not forget. The original inflammation may be so intense that it will occupy,—absorb, as it were, the whole energy of the vital principle. The system shall be so oppressed, overpowered, as to be insensible to everything else. Then I must somewhat abate the original inflammation by other means, before I can hope to derive any benefit from my counter-irritant; and, more than this, it is probable that if my stimulus is not powerful enough to abate the first, it may do more harm than good, by increasing or even producing general irritation. Therefore I do not blister immediately. I see what will be the effect of my first bleeding, and my sedative medicines; and if I am making some progress I call in the blister to my aid; I prevent the foe from rallying, and perfectly accomplish his subjection.

Therefore it is that our blisters do not sometimes rise. The original inflammation has not been weakened. It is more powerful than any stimulus we can apply; and we augur badly when this is the case.

The way being prepared for the action of a counter-irritant, we have recourse to a larger surface,—there is more chance of a blister, in preference to a rowel, and decidedly so, for our stimulus can be spread over its being applied to the immediate neighbourhood of the original inflammation, and, most assuredly, from the extent of surface which I can inflame, I can apply a quantity of stimulus beyond comparison greater than a rowel would permit me to do. I might insert a rowel in an obscure case, in order to avoid the trouble and the blemish of a blister, but I should be justly deemed an unscientific practitioner if I had recourse to a rowel in preference to a blister, in a plain case of acute pneumonia. In the *latter* stage of disease the blister will not act, because the powers of nature are exhausted. We must repeat it,—we must rub more of the ointment well in,—we must rouse the sinking energies of the frame if we can; the effort, however, will generally be fruitless, and the not rising of a blister, in the latter stage of the disease, may generally be regarded as the precursor of death, and especially if it is accompanied by the livid or brown colour of the membrane of the nose.

Care and Attention.—Pneumonia, like bronchitis, requires anxious watching. The first object is to subdue the inflammation, and your measures should be prompt and decisive. If the mouth continues hot, and the extremities cold, and the nose red, bleed again and again, and that in rapid succession. The good which you can do must be done at first or not at all. When you have got a little returning warmth to

the extremities, you will continue to give your sedative medicines, and without one grain of a carminative or a tonic; and the return of the deathy-cold foot will be a signal for further depletion. The commencement of the state of convalescence requires the same guarded practice as in bronchitis. As many horses are lost by impatience now, as by want of decision at first. If you have subdued the disease, let well alone; guard against the return of the foe by the continued administration of your sedatives in smaller quantities; but give no tonics unless debility is rapidly succeeding; and when you have fairly weathered the storm, and your patient seems to be well, still be cautious; consider the nature and the seat of the disease; the predisposition to returning inflammation, for many a week; let not the lungs be put upon; if the season will permit let a two or three months' run at grass succeed to your medical treatment; but if this is impracticable, put off the period of active work as long as you can persuade your employer to let it be delayed; and after that let the horse return as gradually as may be to his usual employment and food.

PNEUMONIA IN CATTLE.

Although bronchitis is the usual chest-disease of cattle, many cases of pure pneumonia will occur. Working cattle are more particularly subject to it, especially those who after a hard day's labour are abandoned to cold and starvation, or who, the moment they are turned loose, are suffered to slake in the cold brook that thirst by which they had been tormented for many an hour. Cows neglected, or too soon exposed to cold and wet after calving, are often attacked by inflammation of the lungs. It frequently becomes epidemic among them. It occurs oftenest in the spring and autumn, and comparatively rarely in the winter. When a warm spring and premature vegetation succeed to a cold and rainy winter, and at the time that they are first sent to pasture, inflammation of the lungs is very rife among cattle, and especially in low and flat and marshy situations, and where the water long lies stagnant over the flooded meadows. The same ground that gives the rot to sheep disposes cattle to pneumonia.

Symptoms.—The symptoms are analogous to those in the horse; the same rigor, depression, heaving, oppressed pulse, coldness of the ears and legs, and *roots of the horns*, and heat of breath. To these are added diminution of milk, swelling of the teats, loss of appetite, cessation of rumination, and some swelling of the abdomen. A slight cutaneous eruption along the back and sides is a frequent accompaniment of pneumonia in cattle, and which we attentively watch. If the eruption is repelled, the inflammation will become more intense, and probably fatal; if the pustules take their regular course, and break, and desquamate, we augur well. There is rarely any acute inflammation in these animals in which the skin or the subcutaneous tissue does not participate, and that peculiar tenderness about the loins accompanies or succeeds the eruption. There is the same protruded head; the same obstinacy of standing,—the same disinclination to move, and a manner of standing even more peculiar than in the horse, the fore-legs being placed as far apart as possible from the sides. Sometimes there is little cough, at others it is constant and distressing, and the sudden cessation of it is one of the most fearful symptoms that can occur. Occasionally the tumours about the joints, to which cattle are so subject, suddenly appear, and we are pleased to see them; they indicate some metastasis, or possible metastasis of inflammation.

The symptoms of an approaching fatal termination of the disease are a little but not very different from what they are in the horse; the quick, weak, intermittent pulse; stupor; insensibility of the skin; fœtid breath; fœtid discharge from the mouth and nostrils, and more from the mouth than in the horse. The disease is sometimes fatal before the termination of the second day, although occasionally the animal lingers on to the seventh or eighth day.

The appearances after death are thus far different, that pneumonia becomes oftener complicated with pleurisy than in the horse; effusion into the chest is oftener found, and in larger quantity. The liver has often acquired an extraordinary size, but the intestines comparatively seldom exhibit much appearance of inflammation. In some cases the membranes of the intestines are even *less* injected than in their natural state.

After apparent cure, there is often a chronic inflammation of the lungs, or more probably chronic bronchitis, which continues for five or six weeks, and at length destroys the animal, of which I shall say more hereafter.

Auscultation.—The exploration of the chest is not so well understood here, and I believe practised by very few; but the time will come, when here too it will receive the attention which it deserves. The respiratory murmur is not so loud in the ox as in the horse. The walls of the thorax are thicker, the integument is thicker, and there is more interposed adipose membrane; but the sound is sufficiently distinct for our purpose, and the changes which it undergoes will afford us very useful indications. There is one thing however against which we must be on our guard. I have observed again

and again that the subcutaneous cellular membrane in these animals strangely sympathises with every acute disease, and that there is often a peculiar crackling noise, rarely observed in the horse, and not only evident when the back or loins are pressed upon, or when the animal moves, but in every act of respiration. Something of the kind is heard while the beast ruminates; it is most distinguishable about the lower part of the chest. It somewhat resembles the guggling of a bottle. This is re'erable to one of the stomachs. We must be careful that we do not confound the crepitating sound of the subcutaneous texture with that of the lungs during pneumonia, nor the guggling in the rumen or reticulum, with that of effusion into the chest, or the existence of abscess in the substance of the lungs.

Treatment.—There must be the same promptitude and decision. We must bleed even more copiously than in the horse, and until the animal is more evidently affected. The same sedative medicine should be administered, but in smaller doses; but purgatives are here admissible, and highly useful. We have in the Epsom salts one that is sure and not irritating; and probably on account of the interposition of such a mass of cuticular surface as the three first stomachs contain, there is not the sympathy which we observe and lament in the horse: the liver, being in contact with the diaphragm most, and sometimes alone, participates in the inflammation Also, as I have observed when speaking of other chest affections in cattle, setons in the dewlap must supersede the blister:—the skin of the ox is too thick and insensible for the blister to produce its proper effect. These setons may be made to produce irritation and engorgement enough if the root of the black hellebore is used instead of the cord.

INFLAMMATION OF THE LUNGS IN SWINE.

The rising of the lights, the popular term for inflammation of the lungs in the swine, is one of the pests of the stye. I am not aware that it is contagious, and yet in a distillery it sometimes destroys a fourth of the hogs in the course of a few weeks. Some atmospheric agency produces a tendency to pneumonic complaints; and the forcing system which is adopted in the styes speedily gives, to that which would otherwise scarcely run beyond catarrh, a character of intense and fatal inflammation. The distressing cough, the *hearing* at the flanks, and the refusal of all food, will be chief indications of the disease. Bleeding will be immediately resorted to—purging, but cautiously attempted; the Epsom salts, will be the safest aperient, and then the sedative medicine already recommended will follow, but with the substitution of the antimonial or James's powder for the emetic tartar. Two grains of digitalis, six of antimonial powder, and half a drachm of nitre, will be a medium dose. The disease will often run its course with an intensity and fatality only equalled by the inflammatory fever of cattle; our measures must therefore here likewise be prompt. The phthisis or consumption, which is the too frequent termination of the disease, even when the animal weathers the first attack, can be combated only by cleanliness and warmth and mildly nutritious food.

INFLAMMATION OF THE LUNGS IN DOGS.

The complaint, although not frequent, is here singularly marked. The extended head—the protruded tongue—the anxious, blood-shot eye—the painful heaving—the hot breath—the obstinacy with which the animal will *sit* up, hour after hour, until his feet slip from under him, and the eye closes, and the head droops from extreme fatigue, while in a moment he is roused again by the feeling of instant suffocation—these are symptoms that cannot be mistaken. Here, from the comparative thinness of the integument and the parietes of the chest, we have the progress of the disease brought as it were completely under our view. The exploration of the chest of the dog by auscultation is a beautiful and a wonderful thing. As however pneumonia in the dog is so generally connected with pleurisy, I shall defer what I have to say respecting it until the next lecture, except remarking that here too our measures should be prompt—our venesection copious—our purging somewhat cautious, and produced by the Epsom salts and not by aloes—the sedative medicines those just recommended for the swine, and the quantity regulated by the size of the animal.

ESSAYS
ON
ANIMAL ACTION.

By WILLIAM DOBSON, *Esq.*, *Surgeon.*

No. III.

RESPIRATION.

THERE are few who have studied physiology, who have not discovered the greatest discrepancies in the theories of the mechanical action of respiration.

Numerous agents conspire in the performance of breathing. We have to ascertain, as far as is practicable, the relative

value of each, and the manner in which they individually and collectively exert their influence.

By the conjunctive actions of the intercostal, the diaphragmatic, and the abdominal, muscles, respiration is supposed to be performed.

Adhering to the important principle of deducing our conclusions from that unerring basis, anatomy, and disregarding the speciousness of theories, however celebrated may be the propounders, taking nothing for data but such facts as are either palpable to the senses, or based on the most striking analogies,—on these grounds the preceding conclusions are carefully inferred.

THE INTERCOSTAL MUSCLES.

We shall commence with the intercostal muscles. They are situated between the ribs, and the obliquity of their fibres is such that they can mechanically approximate the ribs, and in a manner which a vertical disposition would unquestionably have precluded. Now as I conceive respiration to be an involuntary act, the agents which effect it must be involuntary also. Although this supposition is denied by many, yet we shall endeavour to prove its correctness.

Where are we to expect the nerves to arise which supply the intercostal muscles, and under what system of nerves are they usually classified?

Sir Charles Bell, whose name deservedly ranks high in physiological science, conceives that there is a tract of medullary matter, extending throughout the dorsal region of the spinal marrow, placed between the anterior and posterior columns, but as no one can perceive it, we are justified in questioning its existence. That it may exist, and not be discoverable, is quite possible, but until it is discovered we are not warranted in reasoning on its actions. Dissections demonstrate that the intercostal nerves are derived from the anterior and posterior columns of the medulla spinalis, and they demonstrate, negatively, that they have no other origin; that the intercostal nerves arise most unequivocally from the same nervous masses, continued into the cervical region, from which the nerves of the arm originate, those nerves being destined to incite the arm to *voluntary* movements, and possessed of sensation. They have the very identical conjunctions with the surrounding nerves. Each trunk communicates with the contiguous ganglion of the sympathetic by two or more branches, and I affirm that no diversity of *structure* is recognisable.

These are facts, they are the evidences of the senses, and they are strongly corroborated by the testimony of reason.

Are we not compelled then, by the rules of scientific investigation, to consider the motor portions of the intercostal nerves as strictly voluntary? "If the analogy of two phenomena be very close and striking, while at the same time the cause of one is very obvious, it becomes scarcely possible to refuse to admit the action of an analogous cause in the other, though not so obvious in itself."* With this celebrated philosopher I must decidedly concur. But I am nevertheless aware how deceptive are the senses, how frequently they induce in the mind erroneous conceptions. "We can only reason from what we know."

The idea that the intercostal muscles possess an involuntary power of action, has obtained universal and almost implicit credence; but upon no other ground than that of conjecture. I repeat, if these muscles be supplied by the same kind of nerves as those of the arm, and the mechanism be alike, we cannot consistently withhold our assent to the acknowledgment that their functions are identical. No reasonable mind can possibly arrive at an opposite conclusion, for the correctness of the position is so incontrovertibly established, being founded on the basis of demonstration.

The principal argument which can be advanced in support of the idea that the intercostal muscles are voluntary, is the following, that they are so placed as to be enabled to elevate or depress the ribs. But I am inclined to believe, that in proportion as the chest is increased in its circular diameter by the *elevation* of the ribs and their expansion outwards, will the vertical extent be diminished, and therefore little if any effect can result. When the descent of the diaphragm and the action of the intercostals occur simultaneously, a considerable enlargement of the area of the chest must necessarily be produced. Still I am convinced, that for ordinary breathing an increase of the chest's capacity in the vertical direction by the descent of the diaphragm, will cause ample effects. But in the subsequent essay, this circumstance will be more particularly dwelt upon. It will be asked then, If the intercostal muscles be not agents of breathing, what their office? Why were so numerous a set of muscles produced? Reflecting on the conditions of an animal, both man and beast, we observe that the chest has other offices to fulfil than merely to allow of respiration taking place. One great object which the chest is destined to effect, is *voice*. Capacitating the animal to express

* Herschel's Discourse on the Study of Natural Philosophy, p. 119.—*Lardner's Cyclopædia.*

its varied wants, it is the trumpet of the mind, and sounds the alarm. The intercostal muscles being under the immediate dominion of the will, fix the chest, expand it fully with air, or expel its contents, in obedience to the dicta of the mind. They are important agents in enabling an animal to suppress respiration or accelerate it, as in the efforts of listening and speaking, &c.

The actions of a muscle are contraction and relaxation; but the actions of the voluntary muscles are limited in their duration. They act in obedience to the will, thence soon become fatigued, and the integrity of their actions can be preserved only by the will. Thus they are at rest during sleep, and likewise when from any cause will is abolished, as in apoplexy and compression of the brain, or when the communication between the brain and the muscles is interrupted, either by a section of the spinal marrow or the nerves, or when these are compressed by effusion, or are diseased. If the preceding principle be true, the intercostal muscles can continue their operations only for a very short period: they will cease to act during sleep. To conclude otherwise is so disconsonant from reason, that I am surprised any one could do so who is accustomed to think. As collateral evidence, of these muscles *not* being *involuntary*, the following corollaries may be adduced.

In the infantile period, and even at manhood, the elasticity of the thoracic parietes is such as to admit of a considerable diminution or increase in the chest's capacity, in a circumferal direction. The cartilages of the ribs are soft and pliant, and the articulations free and perfect; and it is observed that the breathing is almost entirely thoracic; obviously dependent on the large abdominal viscera, more especially the liver, which obstruct the play of the diaphragm into the abdomen. But as age advances, the viscera in the abdomen diminish relatively in size; the costal cartilages become denser and ultimately ossified, thence lose their elasticity; the articular movements are restricted, so that in old age the mobility of the ribs is frequently destroyed; the floating ribs, however, generally retain a slight degree of moveableness. In the Museum of the College of Surgeons is a specimen of complete anchylosis of the joints of the ribs, and entire ossification of all parts of the chest, so as to have rendered it a solid and immoveable bony case. It must be admitted, that in this subject respiration had existed without the agency of the intercostal muscles.

In old age any motion of the chest is scarcely visible, but full expansion of the chest at this period is not necessary. The circulation is slower, and breathing is proportionate. As his mental and physical vigour is impaired, less atmospheric air is demanded. The vertical enlargement of the chest appears amply sufficient for all his purposes. His days of oratory and singing, of running and leaping, have now gone by, and his approaching death is evinced by languid actions of the mental and corporeal powers.

We have endeavoured to show that the intercostal muscles correspond in their *anatomical* relations to the system of *voluntary muscles*, and we therefore assign to them *analogous* functions. If they possessed a superadded power, there would exist most unequivocally a superaddition of mechanism. This is an axiom in physiology sufficiently established.

WESTMINSTER MEDICAL SOCIETY.

Saturday, April 28th, 1832.

Mr. Chinnock, President.

Although this was the last evening of the present Session, the attendance was not very numerous, and the array of combatants (for there was little doubt of a renewal of the old skirmish) by no means indicated such an encounter as took place. Before giving a report of it, however, it should be premised that whilst the chief of the anti-contagionists were present, many of those who have taken a prominent part on the opposite side of the question were among the absentees.

CONTAGION OR NON-CONTAGION OF CHOLERA.

The reading of the minutes was followed by some remarks from Dr. Gilkrest, who defended himself from the charge of incorrectness relative to the cases of Cocklin and Donnie.

Dr. Blicke also made some observations on the incorrectness of a report which appeared in a late number of the *Times*.

Mr. Greenwood also made some remarks on the same subject, but the President interfered, observing that the course was not in order. The reports in the public journals could not be made a part of the business of the evening.

Mr. Greenwood complained, that whenever a gentleman rose to speak on the side of contagion an attempt was made to put him down. ("No, no."—"Chair.") Mr. G. then made some remarks on the great discrepancies which existed amongst the anti-contagionists as to the cause and mode of propagation of the cholera. When he looked at facts, he wondered at the denial of contagion. A woman near Ely, having

three children, sent one of them to see a friend at a distance attacked with cholera. The child returned, ten hours afterwards was seized and died, after which the two remaining also experienced an attack of the same disease. Now if this could be ascribed to local circumstances, how was it that the children who did *not* go were not first taken ill? An instance had lately occurred within his own knowledge. A girl, quite well, attended the funeral of a person who died from cholera. After the funeral she partook of some refreshments in the house of the deceased, went home, had vomiting and purging, and died. Her sister went to see her, and *she* also died. Mr. G. then referred to the cases lately adduced in THE LANCET by Mr. Prebble, and thought that, after such evidence, a conclusion against the disease being contagious was proof of a most illogical mode of deduction. He had called the "declaration" of the French surgeons and physicians "hasty;" the government regulations in France showed pretty clearly that such was the case. Opinions were changing *somewhere*, for Marshal Soult had made certain arrangements for the marching of troops, in order, as the official documents stated, "to prevent the contagion of the disease spreading from town to town." The opinion of Broussais, too, who had given a tone to the system of continental medicine, was now in favour of the contagionists. That great physician had lately declared, that he never attended on any individual first attacked in a house, without finding that the disease soon broke out amongst several of the other inmates. The same physician further stated, that he regarded the cholera as contagious as the small-pox. Hufeland, too, of Berlin, had pronounced his opinion, that cholera was really to be classed with contagious diseases. It was at least something to have such authorities in his favour. With regard to a gentleman on the other side, Dr. Johnson, who seemed in the last debate to assume great merit for prescience, there was certainly some reason to doubt the extent of his foresight. Did he not, for instance, at the onset deny the contagiousness of the malady? Did he not *admit* "contingent contagion?" (Dr. Johnson here said "Never.") Dr. J. also denied the identity of this disease with the Indian cholera. As to his prediction of the moderate fatality which was to attend the disease in England, he Mr. G. believed that the verification of that assertion was to be ascribed to the judicious measures of the government, and the institution of the cholera hospitals. In conclusion, he would beg to ask the gentlemen of the "Cholera Association," whether they had as yet made any discoveries respecting the peculiarities of the disease, or found any mode of treatment efficacious. Perhaps Dr. Granville would, as a member, act the part of accoucheur to that little body, and deliver it of any offspring of its researches!

Dr. GRANVILLE objected to such an application. The Society had nothing to do with the proceedings of other Societies.

The PRESIDENT thought Mr. Greenwood's question not improper; but Dr. Granville declined to afford his services on the occasion.

Dr. SIGMOND considered that the general voice was now decidedly in favour of non-contagion, in which opinion he himself joined from conviction acquired by personal observation. Dr. Johnson's doctrines had been greatly cavilled at; but he must say that he considered them very correct, and he was of opinion that the greatest benefits had resulted from them. Dr. Johnson was the first who had directed attention to diarrhœa as a premonitory symptom of the disease, and the importance of guarding against it. This suggestion the Board of Health took up, and it had been acted upon with immense success. The spasmodic effects of the disorder seemed not to have been present in England to any violent extent, whilst in Paris they constituted the most marked symptom, the testicles being drawn up with great force. The idea of a malarious origin, he thought really gained corroboration from the instances of Paris and London. In London we had the noble Thames, with its tides bringing fresh water and breezes, and carrying off the filth of the metropolis, conveyed into it from every quarter. But in Paris there were no sewers, the Seine was a sluggish river with no tide, and the city was without the means of speedily passing off its refuse. Most of the diseases of Paris were gastro-enteritic. In Paris, too, malaria might be expected to move with great rapidity through its clear atmosphere, whilst that of London was clogged with smoke and carbonacious matter that offered great obstacles to the quick-spreading of malaria.

Dr. GILKREST, in reference to the Ely cases, said Mr. Greenwood had advanced what he called "facts," but had forgotten to mention any authority. He made this remark, because a gentleman from Ely had stated that they could not in any way trace contagion there. Broussais had also been adduced by Mr. Greenwood as a supporter of contagion, but if the Society would allow him, he would read an extract from the supplement to the *Constitutionnel* of April 24th, containing Broussais' lecture on cholera, which would throw quite a different shade on the matter. [Here Dr. Gilkrest commenced the extract, alternately condensing and quoting verbatim.] Well,

then, he, Broussais, thinks it probable that the *peste affreuse* which ravaged the world in 1348, was of the same nature as the present disease; that the disease is said to have been preceded, the year before, in many towns in Germany, by convulsive catarrh, or the *grippe*, as it has been called; and as this latter disease prevailed in Paris last year, it was believed by those who were in the habit of observing closely, that the cholera would appear there this year. "In this military" hospital (Val de Grace), said Broussais, "we have had the 'avant couriers,' not properly the *grippe*, as but few cases of the convulsive catarrh have presented themselves here; but about five weeks before the appearance of the cholera, we observed a great *susceptibilité* in the digestive organs, so that they were obliged to withhold several articles which they were in the habit of giving to convalescents, and to renounce medicines which they had been accustomed to give as revulsives in catarrhs and peripneumonies. They had, for instance, been in the habit of giving large doses of *Tartarised Antimony* in the height of the winter, but suddenly it was found that not a single grain could be given to certain patients without serious consequences ("accidens extremement graves"). Two patients in particular were almost without pulse thirteen days after its exhibition, and became exactly like the present cholera cases, except that the pulse was not completely extinct. They were in a state of great stupidity; eyes red; extremities cold; pulse fugitive; vomiting, with frequent stools. The patients continued cold for a long time." Dr. Broussais went on to state, that the epidemic broke out suddenly amongst the poorer classes in Paris, and that the first cases at the Hospital Val de Grace, happened on the 29th March. The following were his own words: "It did not at all appear that contagion existed, for the first persons attacked had certainly not been in communication with persons from England, at least this seems probable. The disease is too "*brusque*" to have been transported in a ship. Had it been brought that way it would have been known. It would have been at Calais or some other port that a patient labouring under the disease would have been landed, and, placed in a house, would have communicated the disease to other persons. Nothing of the kind, however, has been shown to be the case. Although the first attacked do not appear to have received the disease from any body, the following has occurred:—When the disease appears in a house, it almost always attacks several persons in the same dwelling.

I do not know an example of its having been confined to one individual only. I do not doubt that instances have occurred, but at least I do not know of many instances to the contrary. When one is called to a patient, the next morning, or again the day after, two, three, or four, are found to be ill of the disease in the house. This would give rise to a suspicion of infection, ("Hear" from the contagionists)—that the disease is communicated from the patient to the attendants. But, on the other hand, the persons in the house may be considered as existing under the same influence—("Hear" from the *antics*), and consequently as contracting the disease independent of all contagion. At the same time it attacks persons living on different floors—different families whose mode of life is different, so that it would appear that in these houses something existed which multiplied the chances of an attack." Here Dr. Broussais quotes instances to show, that in the manner in which the disease spreads there is something very extraordinary. "It may seem to be transmitted by the air, but how admit this when we see it regularly distributed on the same plain, attack one village, and spare a neighbouring one, whilst there is a constant communication between the inhabitants? Those persons, too, who have, without suffering, visited the focus of the disease, become affected in their turn when the disease reaches their place." Now, then, said Dr. Gilkrist, for Mr. Greenwood's observation, or rather that part of Broussais's lecture in which he delivers the opinion by which that gentleman must have deceived himself; what says this continental physician? "From all these facts I really do not know whether I should admit what is called *infection*. As to *contagion*, it is not possible to admit it, if by this be understood contagion similar to what takes place in small-pox." Here he quotes instances of persons having been inoculated with the blood, wearing the clothes of, and sleeping with, cholera patients with impunity. In his second lecture he remarks, that the epidemic cholera morbus "is certainly propagated by a kind of *infection* difficult to characterize," — referring to the preceding explanation. Thus, said Dr. Gilkrist, we have done with Broussais; and as his authority has been granted, of course we may now claim all his weight on our own side.

Dr. Webster in reply to Mr. Greenwood's observations as to the check put on the disease by the Board of Health, said that during the great plague the regulations were much more strict than those adopted for the cholera, and yet the fatality was very different. The posting of the large red cross on the infected house, with the words, "Lord have mercy upon us," and the order to chew valerian and smell stimulants, were quite forgotten.

Dr. JOHNSON complained of the difficulty to please all parties. He must condole with his opponent upon his fortitude in standing out, now that he was losing the allies in whose support he had hitherto been strong. THE LANCET and the *Cholera Gazette* now ceased any longer to advocate contagion. The *Cholera Gazette* had died—not of cholera, but the want of it; not of contagion, but the want of people to support contagious principles. As to THE LANCET, it had turned completely round on the question, and now said that, after watching the disease from the Banks of the Ganges to the Weir, and considering it in that journey to have travelled certainly by contagion, the cholera was become a regular puzzle. One of the arguments adopted by that journal was a singular one; for it contended that it was probable that the epidemic influence actually becomes a preservative against contagion. After this, Mr. Greenwood might make all the use he could of the opinions of Hufeland and others.

After some remarks from Mr. Hunt and Mr. Samuel,

Mr. KING rose to say, that although opposed to Mr. Greenwood in argument, he did not deem him incorrect in stating Broussais to be a contagionist. This opinion he founded on the difference between the English and French version of the word "*infection*," contending that the French understood by it a modification only of contagion. If, after a long residence in France, he might be allowed to pass an opinion on the subject, he should say that Dr. Gilkrest had wrongly interpreted that part of Broussais' discourse. (The remark led to a long philological altercation, which ended in each speaker retaining the opinion with which he started.)

Dr. GRANVILLE, now at the close of this six months' debate, rose to put a finishing stroke to it, having been the original mover of the discussion. The whole circumstances of the disease had been raked over and over again, and the contagionists had become so evanescent, that only one voice was heard in favour of their doctrine, and now that the greater part of the Society were decidedly anti-contagionists ("*No, no*"), he thought that they ought to come to some specific declaration, from which the public might derive confidence. He would venture to propose a declaration against contagion, which might stand on the minutes as a record of his individual opinion, or have attached to it the names of those members who agreed with him.*

The PRESIDENT said that such a proceeding was out of order. The individual opinion might be recorded, but previous to adopting the other proposition, a special notice of such motion was required.

Dr. GRANVILLE persisted in his previous opinion, and gave the substance of the declaration proposed, as stated hereafter.

Dr. JOHNSON approved the proceeding, if allowable, and thought that every man with courage and honesty would come forward and sign such a document.

The PRESIDENT then read an extract from the rules, declaring that a week's notice was required to be given to the secretary, previous to such a motion. (In the propriety of this arrangement a great number seemed to concur), but

Dr. GRANVILLE cited as a precedent the proceedings adopted when the medical corporations were called upon to legislate for the practice of midwifery.

Dr. WEBSTER thought it best, as on the Anatomy Bill, to move the previous question.

Dr. FERGUSSON considered that Dr. Granville would be more correct in moving for an extraordinary meeting, the more especially as so many members were absent. ("That can't be done, as this is the last night.") Well, he would only say, that as there were many contagionists belonging to the Society, no doubt there would be found among them a Duke of Wellington to adopt a "*protest*." Was it wise, therefore, that the "declaration" should go before the public with such a rider? Would that inspire confidence?

Here a pause ensued, when

Mr. GREENWOOD, being informed that he had the right of reply, went over the objections which had been urged against him. Dr. Webster's observations respecting the regulations adopted in the plague, were, he said, well answered by the remark of De Foe, that in isolating the houses you shut up the dead, dying, sick, and healthy, altogether. There was some difference between this, and removing the sick to a place set apart for them. Dr. Johnson had accused THE LANCET of apostatising, but how was he borne out in so doing, when, in the Number for that day, cases were brought forward as mathematically proving infection to have occurred?

Dr. JOHNSON said those cases were adduced by an individual in THE LANCET, not by the Editor.

Mr. GREENWOOD.—No matter. They were noticed as proofs in the leading article. As to the attempt of Dr. Granville to make, or to obtain a "declaration" on the present evening, he could only denounce it as a *ruse* of the party, when many of the contagionists of the Society were absent.

* In the course of this speech Dr. Granville indulged in some remarks on what he was pleased to call "the weathercock changes of THE LANCET." We allude to this without burdening our columns with the iteration of "arguments" on other matters in which the remark was included.—ED.

The contest for a "declaration" was again commenced amidst much confusion, and occupied a very long time. The President protested three times against the unfairness and irregularity of proceeding, but the movers persisted in "dividing the house." The house accordingly was divided, the contents ranging on one side of the room, and the non-contents on the other, when the following declaration was read from the chair.

"That the Westminster Medical Society, having devoted the uninterrupted space of six months to the serious and dispassionate consideration of the malady which has been prevailing in England since the latter end of September last, and especially in the metropolis; and having heard the several arguments, doctrines, and facts of the many members practically as well as theoretically engaged in that important inquiry, declare that, in the opinion of the majority of the Society, the evidence brought forward to prove the said malady a *contagious* disease has completely failed, and that every circumstance which has come to the knowledge of the Society shows the disease in question to have begun, progressed, and ended, in the ordinary way of every other epidemic disease."

After this a new point of contention arose in the shape of an amendment to *adjourn*. This being negatived by a majority of *one*, "strangers" were required to ascend into the gallery, and the division on the floor below gave

	Members.
For the declaration	24
Against it	22
Majority	2

The CHAIRMAN, and Dr. STEWART, as the other President, again entered their protests, and adjourned the meeting to that day six months.

Thus ended the CHOLERA DISCUSSION, and the session of 1831-2.

LATE PROCEEDINGS AT THE WESTMINSTER MEDICAL SOCIETY.

To the Editor of THE LANCET.

SIR,—Saturday, the 28th of April inst., was the last meeting of this session of the Westminster Medical Society, and at the conclusion of the debate on the contagious or non-contagious nature of the malignant cholera, Dr. Granville presented a declaration to the President of the Society, which has since gone forth to the world in the columns of the *Morning Post* and *Morning Chronicle* newspapers, as containing the sentiments and opinions of this enlightened body, that "The evidence brought forward to prove this malady (malignant cholera) to be contagious, has completely failed." The President very properly stated that this proceeding of Dr. Granville was irregular, and contrary to the standing rules of the Society, it being one of the rules, that due notice shall be given when the collective sense of the Society is to be taken on any particular subject. Notwithstanding this announcement from the President, the declaration was put in the midst of much noise and senseless clamour, 46 members only being present. 24 voted for it, and 22 against it, leaving a majority of 2 in favour of this precious declaration. Now the members of the once far-famed Cholera Association had collected all their strength on this occasion, but the great majority, and really intelligent portion, of the members, were absent, more particularly those who entertain a directly opposite opinion to that which this declaration puts forth, they not expecting that a subject of such importance to humanity would be disposed of by a mere "ruse de guerre," so unworthy of a grave and scientific body. Professor Burnett, Mr. Lambert, and several other members present, expressed great disapprobation at this unusual mode of proceeding. After this, yourself and readers will, I have no doubt, justly appreciate the merits of the declaration as it deserves. I do think, myself, that a meeting extraordinary ought immediately to be convened of the great body of the members, to take into consideration this marked violation of their rules. I am, Sir, your obedient humble servant,

JAMES GREENWOOD.

9, Bentinck-street, Manchester-square,
May 1, 1832.

Substance of the Investigations regarding Cholera Asphyxia, with Cases and Dissections, communicated by Professor Delpech, &c., to which are added, Observations on the Disease in Edinburgh, and the neighbouring Districts, with numerous Cases and Dissections. By JOHN LIZARS, &c. 8vo. pp. 72. Edinburgh: John Hamilton.

THE first part contains a letter from Monsieur Delpech to the author, detailing some observations on dissections made by him at Musselburgh and Glasgow. This letter has already appeared before the public, and the opinions of M. Delpech have been fully discussed in this journal. We

shall now only add, that the correctness of the observations then delivered has been fully confirmed by the results of several dissections made in Paris since the appearance of the disease in that capital. The second part of the pamphlet, by far the most valuable, and deserving in the fullest degree all the praise we can bestow upon it, comprises an account of eleven dissections performed either by Mr. Lizars, or some of his immediate friends. Of these we are able to say generally, that they have been conducted with the utmost care, patience, and anatomical skill. The summary of twenty dissections gives, according to Mr. Lizars, the following results:—

"SUMMARY OF TWENTY DISSECTIONS.

Brain.

Of this number twelve had this organ examined, and, in all, the arteries and veins of the integuments and muscles covering the cranium were distended with the dark blood, which, in some, flowed like tar.

In ten, the blood-vessels of the dura mater were turgid with this blood; and in three, there were fibrinous coagula.

In seven, there was serous effusion under the arachnoid membrane.

In four, the pia mater was congested with blood-vessels.

In seven, the cerebrum was highly vascular; and in one, slightly softened.

In seven, the cerebellum was very vascular; and in three, its substance was slightly softened.

Spine examined in Ten.

In six, serous effusion between theca vertebralis and arachnoid membrane, and, in one of these, the fluid was bloody.

In two, serous effusion between arachnoid and pia-mater.

In six, blood-vessels of spinal chord highly injected with the dark blood; and one with evidence of inflammation between dorsal and lumbar regions.

In six, the spinal or rachidian veins turgid with dark blood.

Ganglionic System examined in Seventeen.

In ten, the neurilema of pneumogastric nerves was injected with blood-vessels; in one, the nerve was enlarged; in another, it was thickened; and in a third, the neurilema was inflamed with ecchymosed patches.

In six, the neurilema of splanchnic nerves was vascular; in two, the ganglia at their origins were vividly injected; and one ganglion was ecchymosed.

In sixteen, one or both of the semilunar ganglia were vascular; in one, it was inflamed; in three, it was enlarged and infiltrated with blood or serum; and, in two, softened.

In eight, the solar plexus highly vascular throughout; in three, the ganglia and nerves enlarged, and one infiltrated.

In four, the renal plexus was very vascular.

In four, the œsophageal plexuses were vascular.

In one, the recurrent of the pneumogastric nerve was vascular.

In five, the cardiac plexus was enlarged, and very vascular.

Thorax.

Heart.—In three, the heart was flabby and pale; in two, collapsed; and many of them had the left ventricle so contracted and firm, as to contain only a drachm of blood. In thirteen, the right side was full of the dark gory blood, part of which was generally in the state of a fibrinous coagulum.

In three, the left side was full of the same blood with coagula.

In three, the right auricle full of dark blood and coagula.

In six, the left auricle full of dark blood and coagula.

In four, left ventricle was moderately filled with blood and coagula, and one affected with softening; in two, coagulum extended into aorta.

In five, right ventricle full of blood and coagula. In one, coagulum extended into pulmonary artery. In two, the parietes were softened.

Pericardium.—In one, this sac was distended with gas; in two it was dry, like paper, and vascular; and, in a third, dry, vascular, and diaphanous. In four it was vascular. In all, the coronary vessels more or less injected with dark blood.

Venæ Cavæ.—In all, more or less of the dark blood was found.

Pulmonary Veins.—In six, these veins were turgid with the dark blood.

Lungs.—In four, these organs were congested with the dark blood.

Pulmonary Artery.—In one, a large coagulum, which extended into its two large branches. In three, it was full of the dark blood, and in three others the vasa vasorum were highly injected.

Pleura.—In five, highly injected; and in two, there was effusion of lymph.

Aorta.—In all, it contained more or less dark blood, with fibrinous coagula; in six, the vasa vasorum were highly injected—the dark blood, and occasionally coagula, extended into the carotid, brachial, femoral, tibial, ulnar, and radial arteries.

Abdomen.

Peritoneum.—In nine, this membrane was highly injected; in six, evidently inflamed; and in three, there was albuminous effusion, with some turbid serum. In one, the omentum was very vascular, and in another it was inflamed.

Stomach.—Generally of a white colour, both on its peritoneal and mucous tunics, and containing more or less of the rice-water fluid. In seven, there were distinct vascular patches on the mucous coat, with several ecchymosed spots, varying in size from that of a sixpence to that of a half-crown, and in all there was manifest softening. In one, the mucous tunic was eroded.

Small Intestines.—In twelve, there were evident marks of high inflammation, and vivid and extensive injection; in nine, ecchymosed patches; in four, mucous tunic softened in many points; and in one, incipient ulceration. Contents of a viscid white mucous, or greenish colour, and in two they were bloody.

Large Intestines.—Transverse arch and sigmoid flexure of colon, commonly spasmodically contracted. In five, vascularity, with ecchymosis. Two inflamed, with softening, and one with ulceration. Two with dark venous congestion, similar to intestine in strangulated hernia. Contents generally rice-watery, or gruelly and flocculent, occasionally greenish and viscid. In many the colon, with the exception of the caput cæcum, was empty.

Liver.—Very various in colour; two with Bright's yellow deposit. In some the vena portæ were moderately congested, and in one the biliary ducts full of bile.

Gall Bladder.—Generally two-thirds full of rather inspissated olive-green bile. In the twenty cases ten were full of this fluid, the others varied from a little bile to two-thirds.

Pancreas.—Generally healthy.

Kidneys.—Commonly healthy, but varying like the liver, according to the habits of the individuals. Three were slightly congested, one gorged with the dark blood, and another presented a livid appearance.

Urinary Bladder.—In all contracted, and almost empty. When any fluid was present, it was about a drachm of mucopurulent. One however was contracted horizontally, and contained five ounces of limpid urine."

The third portion of the pamphlet contains Mr. Lizars' "Theory of Cholera Asphyxia." We shall endeavour to collect his ideas on the subject, but must confess at the outset, that many passages, apparently embracing the pith and marrow of his doctrines, are to us perfectly unintelligible. Mr. Lizars is a good anatomist, but he expresses himself in very obscure and unscientific terms when physiology or pathological induction form the subject of his discourse. The atmosphere, according to Mr. Lizars' theory, is unseasonable, and predisposes the constitution of the individual for some time before he is attacked, by impairing the whole nervous system, but especially the ganglionic system of nerves. A deficiency of nervous energy to the important organs supplied by the ganglionic nerves takes place, the chylopoietic viscera become deranged, and the individual is thus prepared, on exposure to any exciting cause, as cold, &c., for the reception of the disease.

"The expansive cutaneous filaments of the nerves are now *chilled* and *excited*; the cutaneous capillary exhalants are shut up; these extreme branches of the nerves and arteries produce a concentration of the nervous and circulating systems; re-action is attempted, and on its failure, a determination is produced to the ganglionic system of nerves.

An *inflammatory spasm* of these nerves and their organs is the result; and hence the sluggish respiration and circulation, the decomposition of the blood, &c.

The vomiting and purging are consequent on the inflammatory spasm combined with an overloaded condition of the whole alimentary canal, which must be severely irritated by these ingesta; and hence diarrhœa is almost always a precursor of the violent symptoms.

To prove that inflammatory spasm is the cause of the vomiting and purging, (independent of what has been seen in dissection,) we find that, as the disease advances —as the inflammation continues and destroys the nervous and circulating systems supplying these organs—so the vomiting and purging, or dejections, cease.

The cramps are consequent on the same inflammatory spasm being communicated through the medium of the ganglionic nerves along the arteries, and from the communication of these nerves with the spinal chord and nerves of volition. This is beautifully but painfully exemplified in Mrs. M——'s case, where pressure on the abdomen produced tetanic convulsions of the muscles of the neck and also of the lower jaw.

The blueness of the skin is consequent on the blood not being propelled, with its accustomed vigour and frequency, through

the lungs by the heart, and also and chiefly by its not being duly oxygenated.

The coldness is consequent on no caloric being generated in either the capillary pulmonary tissue, or the cutaneous capillary tissue, and by the blood not being arterialized, but remaining carbonized."

The blueness of the skin and coldness are consequent on the diminished action of the heart, the non-oxygenation of the blood, and the non-generation of caloric in the cutaneous or pulmonary capillary tissues.

Such is an abstract of Mr. Lizars' theory; but we have read the original over and over again without being able to comprehend what he means by chilling and exciting the cutaneous filaments, or by his inflammatory spasm of the ganglionic nerves; physiologists have commonly thought, that chilling the expanded extremity of a nerve produces an effect quite opposite to excitement; and inflammatory spasm, communicated along the arteries to the muscles, and thus producing cramps, purging, &c. is an expression to which we can attach no definite idea whatever. It is remarkable also, that Mr. Lizars, in common with Loder and Delpech, has neglected to point out what the anatomical condition of the sympathetic is indicative of in this severe affection. His expressions all point to functional derangement, not to organic change; in one part indeed (p. 45), he speaks "of a determination to the ganglionic system of nerves;" but the reader is unable from the context to decide whether he alludes to a determination of blood, or of nervous energy. Let us, therefore, turn to the general summary of appearances to solve the problem. What is the pathological cause of cholera? Is it some morbid affection of the central system of ganglionic nerves, as Delpech, Loder, and Lizars think?

We may remark, in the first place, on the vagueness of the phrase, "morbid state," "morbid condition." Is it inflammation, hardening, ramollissement, infiltration of the substance, or all or any of them together? Are we justified in concluding that inflammation of the semilunar ganglia will give rise to the same external symptoms, as simple and frequently slight venous injection of their neurilema. Again, it may be asked, Is this an appearance universally found? For on this the whole question hinges. Abercrombie has remarked, that the progress of medicine, as an inductive science, is retarded by the construction of hypothetical theories, or the assumption of principles which are altogether gratuitous and imaginary, and also by the deduction of general principles or conclusions, from a limited number of facts. The same scientific writer wisely advises medical inquirers to restrict their investigations to ascertaining the universality of a fact. Let us then try the theory of Mr. Lizars by this test, and from his own testimony, "The ganglionic sytem was examined in seventeen subjects. In sixteen, one or both the semilunar ganglia were vascular." Granting this vascularity of semilunar ganglia to have produced the disease in sixteen, what was the cause of death in the seventeen? What becomes of the universality of the fact, and of Mr. Lizars' theory? The indeterminate meaning of the word vascularity has been noticed already, and receives a good commentary from the account of the tenth case, from which we extract the following passage:—

"*Nerves.*—Par vagum, splanchnic, and semilunar ganglion on each side, examined, but no very unusual appearance observed in them, except that the small vessels supplying them partook of the general injection; lungs of rather a dark colour."

This, we conscientiously believe, affords a key to many of the other cases; the injection of the neurilema was not a mark of specific affection, but only one of many contemporaneous events. The agency of the injection of the nervous neurilema has been assumed by Reil,* as the efficient cause of typhus fever, because he found this appearance in many of the bodies which he examined. The parallel is close, and a short period will suffice to consign both theories to the same tomb.

Hyperanthraxis; or, the Cholera of Sunderland. By William Reid Clanny, M.D., &c. 8vo. pp. 149. Whittaker: London, 1832.

Of all the plagues and pestilences of modern days, not even excepting the multinominal yellow fever, we scarcely recollect

* Fieberlhre Band, iv. p. 56.

another so oft baptized, and with all so inappropriately designated, as *the* cholera. We mean that of Asia, of Warsaw, of London, of Paris, &c. For in order that we should clearly understand of what we are writing, we must combine the term with some such local appendix, just as the crowds of synonimous Mackenzies, and Frazers, and Macconochies, in Caledonia, assume the "title" of their estate, kail-yard, or other holding, as a necessary mark of distinction in society.

Already in an article of some length, we have taken the liberty of impeaching the earliest sponsors of this disease, for the extraordinary contradiction which the name they gave it, afforded to its most prominent symptom. Strange as it may seem, though this absurdity was generally acknowledged, every subsequent reformer still fell into an analogous error. A few examples of these misnomers we may repeat, if not for the advantage, at any rate for the amusement, of our readers. MARCUS, of Moscow, for instance, having performed a cholera autopsy, and found the heart flaccid and enlarged, forthwith wrote an essay to prove that the heart was the seat of the disorder, at the same time naming it "*Cardiogmus lethalis!*" M. SCIPION PINEL, having, after similarly extensive experience in Warsaw, found marks of inflammation of the trisplanchnic nerves, invented the term "*trisplanchnitis*," as the substitute so much desired. A third luminary of great literary and etymological notoriety, contended that "diarrhœa *cephalica*" (!!) should supersede all other appellations—and now we find our friend Dr. CLANNY, with almost unparalleled euphony, fishing in the depths of classic lore for a name, and hooking out, after many laborious experiments, much contemplation, and very cautious induction, the epithet "hyperanthraxis," which he explains in these introductory terms:—

"I have ventured to give an appropriate name to this pestilence, to which I am prompted by my own experiments. I expect that it will be received by the medical faculty instead of "cholera," literally a flowing of bile, though in this disease when formed no bile flows as far as I have been able to ascertain from personal observation. 'Hyperanthraxis' ex ὑπερ super, et ἀνθραξ carbo, Anglice, excess of carbon."

Our readers doubtless experience a degree of curiosity fully equal to our own, to ascertain the experimental facts on which this assertion is founded. By these, of course, our estimation of the peculiar opinions which the author subsequently advances in connexion with the carbon hypothesis, must be guided. They are contained in the following passages:—

"After much reflection upon the subject of FREE carbon, I devised and carried into effect the following method for discovering its proportion in the blood of different individuals; and if a multiplicity of experiments which uniformly accorded, not only with sound pathology, but also with each other in their results, be considered as deserving the confidence of the profession, I feel satisfied that this plan of investigation will be found not only worthy their approbation, but also suitably convenient to such practitioners as may not be in the habit of performing chemical experiments.

"Having carefully collected the colouring matter in the manner which I formerly published, and having dried and powdered it, I place it in a crucible, to which is closely fitted a cover, out of the centre of which issues a small tube with a stop-cock. The apparatus is made of platinum, and is perfectly air tight.

"Having a charcoal fire in the furnace in readiness, I fill the crucible with the powdered colouring matter, after which I remove it to the scale to weigh it. When it is returned to the crucible, leaving the stop-cock open, I gradually heat the crucible till the gases are extricated. I now shut the stop-cock, continue the process till the crucible arrive at a white heat, and keep it in that state for a few minutes. I now remove the crucible from the fire, and find in the crucible when cold, pure carbon and a bulky cinder. I wash, dry, and weigh the carbon.

"I weigh the cinder, break it into fragments, submit it again to a white heat, and afterwards collect the carbon, if any, and add it to the above-mentioned quantity. In this way I meet with no difficulty in ascertaining the nature of blood as to its FREE carbon, whether the patient be affected with epidemic cholera, or any other disease. * * * * I added to the platinum tube a bent brass tube, and in a quicksilver pneumatic apparatus, collected the gas from the colouring matter of the blood as it arose. Upon analysis I found that this gas was composed of carburetted hydrogen three parts, and of carbonic acid one part."

In commenting on this process, we ad-

dress ourselves especially to those who are conversant to a certain extent with chemical literature and manipulation. To such we shall satisfactorily prove, that these experiments not only totally fail in showing FREE carbon in the blood, but that they are inconclusive in every other particular.

It is necessary in the commencement to state, that according to the researches of Brande, Engelhart, and Michaelis, whose experiments have established the fallacy of many of Berzelius' opinions relative to the colouring matter of the blood, that principle consists of an animal matter, associated with iron in a minute quantity, and with some earthy salts. The animal matter according to the minute and excellent analysis of Michaelis consists of, in venous blood—

Carbon....	53. 231	Hydrogen	7. 711
Oxygen ..	21. 666	Nitrogen..	17. 392

The precise mode in which these elements are arranged, whether in binary, ternary, or quaternary combination, has never yet been shown, nor is the problem indeed one of the least importance. Previous to the researches of Dr. Clanny, however, no *chemist* ever pretended that one of these four elementary substances existed in a *free* state, that is, as oxygen, hydrogen, nitrogen, or carbon, not combined with one or more of the others. Now Dr. Clanny states, that he has proved the existence of *free* carbon, by burning the colouring matter in a close vessel, and finding a carbonaceous residuum in the retort, and *carburetted hydrogen*, and *carbonic acid*, in the receiver.

The following remarkable faults occur in this analysis:—

In the first place, Dr. Clanny only gives carbon, carbonic acid, and carburetted hydrogen, as the products of his experiment, whereas colouring matter containing *nitrogen*, that gas, or some of its compounds, such as ammonia (hydrogen and nitrogen) or cyanogen (nitrogen and carbon) must be procured if the process be correctly performed. But neither nitrogen nor any of its compounds having been obtained, we are forced to conclude that the analysis was altogether imperfect.

In the next place, animal matter, when subjected to this destructive distillation in close vessels, always yields empyreumatic oil and water; Dr. Clanny obtained neither of these inevitable results.

Thirdly, carbon being the only *fixed* ingredient of the elements of the organic portion of colouring matter, it must remain behind when exposed to heat. Consequently a carbonaceous residuum in the retort is no proof of *free* carbon existing in blood, any more than the obtaining of common charcoal by distilling oak-wood in close cylinders, proves that FREE carbon existed in the pieces of timber before they were burned.

Fourthly, The quantity of carbon in the colouring matter having been proved by Michaelis to be 53¼ parts per cent. in standard or healthy blood, great excess in the proportion of that element is a *natural* fact totally unconnected with disease.

Fifthly, Even the results of Dr. Clanny's experiments, compared with the standard just cited, indicate not an excess, *but a deficiency, of carbon*, in cholera blood. Thus he finds in one specimen of cholera blood,

Colouring matter .. 253 per 1000 parts;
Free carbon ..... 66 per 1000;

which gives a proportion of one-fourth, and a fraction, $\frac{3}{4}\frac{3}{8}$ only, to the sum total of the colouring matter,—being not an excess, *but a deficiency of more than 25 per cent.*

The preceding comments, and we might multiply them much farther, are sufficient to show that the experiments of Dr. Clanny are not conclusive—that if they prove any thing, they prove the opposite of his opinions, and that the name he has furnished for the disease must be rejected.

Having thus demonstrated the premises of the author to be altogether erroneous, we cannot of course take into our estimation any of the conclusions which he derives from their consideration.

"I have witnessed (*Lizars' Pamphlet*) many a contagionist medical man feeling the skin and pulse of the patient with gloves on his hands, a handkerchief over his mouth, and a scent-bottle at his nose, and stopping as short a time as possible with him. To do justice to a cholera patient, the medical attendant must remain with him for an hour or two, and put his own hand to the work. Can the contagionist do so?"

Simple man! Yes.

THE LANCET.

London, Saturday, May 5, 1832.

The recent epidemic, having in its sojournment in this city gone far to disprove many of the hypotheses to which it had before given rise—having baffled alike the theories of the aërialist, of him who placed his faith on terrestrial emanations, of the animalculist, the electrician, and the astronomer, has shown us at least thus much, that while we must admit the operation of a powerful contagion, and while some circumstances denote the simultaneous action of an unknown and more general agent, there was at any rate a state of constitution abroad which defended the mass of our citizens from either, or both, of these intrinsically destructive poisons.

Take up which side of parties, which view of debated questions, we may, let us become for the moment the most rancorous of the non-contagionists (if any such remain) or the most raving of the *lunatics* who blame the fair moon for the working of all this mischief, one thing is at least clear and palpable, that it is quite as difficult to understand why of twenty persons residing in the same house, and under apparently similar external circumstances of air, and food, and raiment, habits, health, and occupations, two should suffer, and eight-and-twenty should escape. If this circumstance opposes the opinion of contagion, it is plain, that it is quite as adverse to any other speculation concerning the causes of the disease. These inferences then we must all adopt, that the causes of immunity or insusceptibility on the one hand, and of the predisposition so remarkable on the other, form a problem for investigation, at the same time most attractive in its nature, and the solution of which is most pregnant with one advantage to mankind, of all the questions which have ever engaged the attention of the medical philosopher.

To examine inductively the nature of the causes which protected the citizens of London from the ravages of the cholera, and the absence of which laid open the inhabitants of Paris to its fury, or conversely to examine the nature of the causes which favoured the operation of the pestilence in Paris, while their absence diminished its mortality in London, are questions of which our scientific societies and the medical journals might undertake the discussion with much advantage. Although an absolute solution of the question might not be arrived at, this good would at least be accomplished, that provided the discussion were conducted according to the rules of common reason and common sense, and in the mode we shall presently point out, the influence of many agents now held to be of great importance would be correctly ascertained, and we should learn at least the amount of our own ignorance on the subject.

An investigation of this kind should, we consider, be thus conducted. Supposing a society formed for the purpose, a catalogue should first be drawn up, embracing and classifying all the known external agents by which communities visited by the disease have been surrounded, such as the state and kind of air, food, habits, customs, &c. On each of these heads discussion should take place seriatim. The statistical history of the disease would readily furnish ample materials to decide on the individual questions, and then another head should be taken up. Proceeding thus by the admission or rejection of probable causes, the ultimate question would be narrowed into a circle of very small diameter, and the chances of successfully arriving at the truth would be proportionately encreased.

We have been partly urged to the preceding train of reflections by the following extraordinary, and to us most unaccount-

able, fact. Such of our professional readers as have visited Edinburgh will readily recollect the local position and external circumstances of the fishing parts of the villages of Musselburgh and Newhaven. They will remember the peculiar, almost Flemish, costume of the women, the similar and curious construction of the houses, the apparently identical habits and customs, the approximation of each locality to the Frith of Forth, the similar direction of the streets, and the common occupation of the inhabitants. They will recollect also the extensive intercourse between the two villages, and then they will, perhaps, participate in our wonder at the circumstances, that while Musselburgh has almost been desolated by the cholera, not a single case has occurred at Newhaven!

On the proper investigation of this astonishing fact, we would stake much of the probability of a solution of the questions we proposed. Either *the cause* of the malady must never have visited Newhaven, or the inhabitants were not susceptible of its influence if present. But that the cause did visit Newhaven can scarcely be denied by those who look at the general progress of the cholera in Scotland. Some difference, then, not appreciable to ordinary perception, existed among the *circumstances* or *inhabitants* of the two places, owing to which "one was taken and the other left." What then is the difference? To the discussion of this question we will gladly open our columns, and we invite to it the immediate attention of our Scottish correspondents.

The proceedings at the rookery in Sackville-street have at length terminated, and, luckily for the lungs of the chief ravens, without any of the pulmonary calamities which their perpetual and uproarious cawings gave us reason to anticipate.

Encouraged, perhaps, by the speedy approach of the end of the session, one of the noisiest of the aviary took leave to declaim on this occasion on our "weather-cock" doctrines, and reproach us with veering round to *his* opinions. The reproach does credit to the orator's candour and self-appreciation. We are glad that he can so rightly estimate the amount of respect his opinions should obtain. As, however, he spoke under the influence of misapprehension, and as we quite agree with him as to the little honour to be gained by a participation in his sentiments on the subject, we beg leave to revert briefly to the opinions we discussed in the leading article of the last LANCET, relative to the mode of propagation of the cholera lately prevalent in London.

In that article, in an honest and upright spirit, which the person alluded to evidently could not understand, we adduced, at the same time, irresistible evidence of the operation of contagion; and we also particularised some strong facts indicative, by negative evidence, of the operation of some other cause. Never accustomed to shape, bend, or distort facts to suit a partisan-purpose, we gave both selections their full value, and deduced our conclusions accordingly; and these conclusions we showed, by reference to our very earliest articles, to be fully consistent with the principles we ever promulgated. We ceded not one jot from any position we ever maintained. In our first article we held the disease to be communicable by contagion. In our last we adduced the most powerful evidence in support of the same doctrine. In our first we admitted the operation of epidemic influence at the time when we wrote. Subsequently, never yielding or modifying that sentiment, we maintained that the progress of the malady during the autumn and early winter months of 1831, was inconsistent with epidemic influence at the time, and referable to contagion alone. We hold this doctrine now in its fullest and most determined sense. Lastly; at the return of

the spring, and with the remarkable alterations just shown in the geographical history of the disease, we again deemed it rational to admit the possible agency of a mysterious influence. Where is the apostacy or wavering in this? If the seasons alter, and diseases obey their phases, are we accountable for the event? Are we culpable for keeping an honest record of the things which fall before our observation? We content ourselves, then, with saying, with reference to a charge originating only in the ignorance of our accuser, that we are fully prepared to maintain every opinion we ever advanced on this question. We can do this easily, for our opinions were ever deductions from authenticated facts. These premises still remain for our service, and they admit of no other conclusions than those they have already afforded.

We shall enter into no further argument with this individual. Indeed it is hard to reason with any one who will not take the trouble of studying the subject he declaims on, and who but six months since was so proficient in pathology as to call the petechial hæmorrhage of typhus a cutaneous eruption! and was so skilled in the geography of cholera—that disease which has prevailed on the Himalaya mountains, the Gauts, the Nepaul, and the Caucasus, as to maintain that it was the exclusive tenant of the swamp, morass, and similarly low situations. Recommending this gentleman then to the perusal of a few elementary works on cholera during the summer vacation, we leave the subject for a week.

WESTMINSTER HOSPITAL.

MORTIFICATION OF THE MOUTH FROM THE ABUSE OF MERCURY.

William Somerville, a child about three years old, was admitted under Dr. John Bright, with a gangrene of the mouth and chin. The little patient was brought in by his mother, who stated that this and a younger child of hers being affected with fever, she was induced to consult a young medical gentleman whose washerwoman she was. Twelve white powders were prescribed for each child, to be taken within twenty-four hours. After taking them, the children's faces became swollen, and much water ran out of their mouths, and the breath grew very offensive. In the case of the child admitted, the gums became inflamed and black, and some of the teeth fell out. Three days from the first administration of the powders, a small black spot appeared between the lower lip and chin, this gradually enlarged until it attained the size of a half-crown piece.

On the fourth day, application was made to one of the parochial surgeons in this city, but his numerous avocations prevented him from attending regularly to the case. The symptoms in consequence became more and more severe, and on the fifth day the parents were persuaded to bring the children to the Westminster Hospital.

At this time, the sphacelus on the face of the boy had increased to the size of a dollar, while the adjacent skin, including that of the upper lip, was of a dusky-red hue, becoming lighter towards the external margin of the phlegmonous circle. The subcutaneous veins were turgid, and formed dark-blue zig-zag lines extending over the surface of the throat and chest; all the teeth had fallen out, and the odour was intolerable. The inflammation affected the mucous membrane, there was frequent cough, excessive irritation and restlessness, and the little fellow moaned constantly. His hands were secured, a carrot poultice was applied, and wine and gentle aperients were administered.

March 29th.—The mortification is extending on the right side; the right eye is

closed; countenance sunken and anxious; great restlessness; head constantly moving; thirst; respiration rapid; cough short, dry, and frequent; diarrhœa. A large bread poultice, moistened with a chloruret of soda lotion, is applied, but is with difficulty kept on. Wine and water for drink, opiate and cordial medicine.

Evening.—The sphacelus has spread; anxiety of expression; prostration of strength; occasional restlessness. The child is evidently sinking.

The younger child was attended at home by some of the medical officers of the Hospital, and died a day or two afterwards.

Autopsia twenty-four hours after death.

The boy was not much emaciated by his malady or his treatment. The external slough involved the whole of the chin, under lip, and part of the upper lip. On cutting down upon the inferior maxillary bone, it was found denuded of its periosteum, as far back as the root of the coronoid process, black, and perfectly detached from the muscles and mucous membrane of the mouth. There were no gums on either jaw, and in the upper one a few teeth were found loose in the alveoli. The slough included the integuments and muscles of the entire under lip and part of the upper lip, the periosteum, gums, mucous membrane, and about a line's thickness of the muscles forming the root of the tongue. Here there was an evident line of demarcation. No swelling or redness of the pharynx or tonsils was observable. The epiglottis round its edges only presented a vivid red tint. All the salivary glands were tumefied. The tracheal and œsophageal internal linings were quite unaffected.

The lungs appeared to have been formerly subject to inflammation, for several solidified nuclei were discovered pretty equally dispersed throughout the substance of both these organs. The ramifications of the bronchiæ were pretty generally filled with mucus, but no injection of their inner tunic existed. The heart was healthy.

In the abdomen there was a disease of the mesenteric glands, and all the organs were apparently in their natural condition. In the head there was no morbid appearance, except a slight injection of the vessels of the pia mater, most perceptible over the medulla oblongata and upper portion of the spinal marrow. No opportunity was afforded of opening the body of the other child.

Gangræna oris is a disease of which but very meagre accounts are to be met with in the annals of medicine. It is to be feared, however, that its occurrence is far from infrequent, especially in parochial and dispensary practice, where the inadequacy of remuneration to medical attendants renders instances of neglect inevitable. In parishes the medical attendance of the poor is *farmed out* in the most unprincipled manner, the appointment of surgeon being almost invariably given to the lowest bidder. In the country this is a greater evil even than in town, for it is not uncommon to find medical men contracting to give medicine and advice to a whole parish for a sum of 10*l.* sterling per annum. In the more populous parishes of London, we believe that occasionally as large a sum as 100*l.* per annum is allowed to the medical attendant for his services. But no surgeon can be expected to furnish himself with the common necessaries suitable to his station in life from such a sum, and the demands of his private practice upon his time leave but little of it to be devoted to pauper patients.

Those pseudo-charities, called dispensaries, are even more baneful in their effects than is the system of parochial doctorships, for in nine cases out of ten they originate as medical speculations. A few of the more impudent amongst the adventurers succeed by this means in raising themselves to a certain degree of notoriety, if not of reputation, heedless of the injury they inflict on the interests of their more honest professional brethren. The sick poor, in fact, will never be properly treated until after a perfect reform of the poor laws, under which medical officers of acknowledged ability may be ensured a sufficient remuneration to allow of their devoting the whole of their time to parochial medical duties.

To return to the subject more immediately before us. There is hardly to be found on record a single case of gangræna oris in children, and ascribed to its true cause, the excessive administration of mercury. In the *Dictionnaire des Sciences Medicales*, some cases of sloughing in adults, after the exhibition of this mineral, are stated by Cullerier. Baron Van Swieten, in his Commentaries on Boerhaave, notices a case in some respects analogous to the one just related, but he refers it to a *scorbutic* origin. A somewhat similar case is also quoted by Dr. James in his Medical Dictionary. The words of Van Swieten are as follows:—

"Si autem multum tumeat gingiva, simulque admodum rubeat validæ inflammationis signum est; quæ quandoque in gangrænam satis cito terminatur; præcipue si acrior humorum indoles simul adsit. Cito autem gingivæ sic affectæ putrescunt, et malum ad vicina loca propagatur, nisi spiritu salis misto cum melle rosarum sæpius tangatur locus gangrænosus cuique sistatur proserpens putredo. *Vidi aliquoties in pauperum infantibus, qui omnino neglecti fuerant,*

partem ossis maxillæ cecidisse, una cum alveolis et dentibus contentis."

A well-marked case of gangræna oris in a child, arising from the abuse of mercury, occurred in the practice of Mr. Broughton two or three years since at the St. George's and St. James's Dispensary, the particulars of which have never been published. A similar case also was met with by Mr. Edwin Thomas, of the Ophthalmic Hospital. Mr. Thomas has copious notes, and intends to give a history of the case in one of the journals. The peculiar sensibility and delicacy of structure of the gums and teeth at this early period of life, throw a great deal of interest on this department of pathology; and there are, doubtless, many intelligent surgeons who possess valuable facts relating to it, which as yet have not been communicated to the public.

SURGEONS TO SMALL VESSELS.

To the Editor of THE LANCET.

SIR,—From the zeal which you have on all occasions manifested in upholding the honour of the medical profession, I feel assured it is only necessary to apprise you of an abuse of a late order from government,—that requiring all vessels sailing with upwards of fifty souls to be provided with a "properly-qualified surgeon,"—in order to find a place for some remarks on it in the pages of THE LANCET. It is expected by many captains that medical men will give their services in consideration of a free cabin passage only being granted for them, and this I am grieved to say has on one or two occasions been acceded to. It is an abuse too flagrant to require comment. Surely, Sir, in medicine of all things, the servant is worthy of his hire, and I leave it to the consciences of honest men to say whether eight pounds per month is too much for the services of a *properly*-qualified surgeon.

For once, Mr. Editor (almost a solitary instance), we have the remedy in our own hands. It is our own faults if we use it not. Let the members of the College, or other properly-qualified persons, resolve amongst themselves, one and all, not to go for less, and we must be successful.

I am, Sir, yours, &c.,

C. E.

SOUTHWARK CHOLERA HOSPITAL.

VERITAS begs us to contradict an assertion which appeared in our last week's journal, relative to the medical attendants at the St. Saviour's Cholera Hospital, Redcross-street (not Winchester-street). "Candour," he says, "compels me to state, that Messrs. Gazelee and Tweedie are not 'indefatigable gentlemen;' at the onset of this disease, they did assist for two days, but since that period Mr. Morley and Mr. Ranking have been actively engaged there: the former as resident surgeon to that hospital, the latter a house pupil with Dr. Whiting, attended without remuneration till he was deterred by contracting the epidemic. Let us adhere, therefore, to the old motto, *Palmam qui meruit ferat.*"*

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

To the Editor of THE LANCET.

SIR,—I shall esteem it a particular favour, if you will allow me to occupy a corner of your valuable Journal in correcting an error which has crept into Dr. Robertson's paper on the salts of morphia in the last Number of the *Edinburgh Medical and Surgical Journal*, through an inadvertency on my part.

Dr. Robertson, in mentioning a communication from Dr. Loudon, describes him as practising in Cheltenham, whereas Leamington is the place which should have been mentioned. Now as this mistake might prove injurious to Dr. Loudon, and as I was not aware of it until too late to correct it in the Journal, or even to insert it as an erratum I have taken this method to counteract any effects which might arise from it prejudicial to Dr. Loudon. I remain, Sir, yours, &c.,

WILLIAM GREGORY, M.D.

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* The above notice should have appeared some weeks since.—ED. L.

TO CORRESPONDENTS.

A Pupil of the Little Windmill Street School must send us his name (in confidence) before his letter appears.

The letter of *Mr. Clay* reached us too late for insertion in *this* number.

The report of the meeting at the College of Physicians next week.

The request of our much respected correspondent at H., relative to a stamped Number of THE LANCET, could not be complied with, because a stamped edition has not been published for many years.

(From a Correspondent.) On Saturday, the 14th instant, a piece of plate was presented to Mr. Alex. Jardine Lizars by his pupils at the Medical School, Brown Square, Edinburgh, on the occasion of his first course of lectures on anatomy, and in testimony of their satisfaction at his peculiar system of teaching that science.

Querist will never get a "satisfactory" answer from *this* quarter. He should apply to his Grandmamma.

Observer. Why so? A thin rat may be worth *killing*, though not worth *skinning*. But MICHAEL's *brains* are not in danger.